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At its meeting on December 27, 1974, the AHA Council authorized the Executive Director to remove the Recently Published Articles (RPA) from the *American Historical Review* and to implement plans for a separate publication. As of this issue, the *RPA* will be published separately in paperbound format. It will continue to appear three times a year, in February, June, and October. The subscription charge for members of the Association will be \$5.00 for the three issues. The charge for nonmembers will be \$8.00, and for institutions, \$7.00. Information concerning subscriptions and application forms can be obtained from the Membership Secretary, American Historical Association, 400 A Street, S.E., Washington, D.C. 20003.

At its meeting on September 28, 1975, the AHA Council authorized the Executive Director to relocate the editorial direction of the *American Historical Review* at Indiana University. The transfer of responsibility is now being effected. The Interim Editor at Indiana University is Robert F. Byrnes; the Interim Associate Editor is Robert E. Quirk. The address is Ballantine Hall, Indiana University, Bloomington, Indiana 47401. The new editorial staff began work on January 1, 1976, and will be responsible for the June 1976 and subsequent issues.

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TIMOTHY E. ANNA is associate professor of history at the University of Manitoba. He has published articles on various aspects of Mexican and Peruvian independence,

including the last stand of Francisco Novella, Mexico City's finances, economic causes of San Martín's failure in Lima, and the process of writing the Peruvian Declaration of Independence. His current research concentrates on the royalist responses to rebellion throughout Spanish America. He has completed a book on the fall of the imperial regime in Mexico City and has in progress another on the viceregal government in Lima. He obtained his Ph.D. at Duke University in 1969, where he studied under the direction of John Tate Lanning.

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Contents

VOLUME 81 • NUMBER 1 • FEBRUARY 1976

Presidential Address

- History as a Moral Science, BY GORDON WRIGHT 1

Articles

- Coming to Terms with Samuel Adams, BY PAULINE MAIER 12
- The Last Viceroy of New Spain and Peru: An Appraisal,
BY TIMOTHY E. ANNA 38
- The Last Whig Historian and Consensus History:
George Macaulay Trevelyan, 1876–1962,
BY JOSEPH M. HERNON, JR. 66

Reviews of Books

GENERAL

- MICHAEL BARKUN. *Disaster and the Millennium*.
By Anthony F. C. Wallace 98
- JOHN T. NOONAN, JR. *Power to Dissolve: Lawyers and Marriages in the Courts of the Roman Curia*.
By Henry G. J. Beck 98
- WILLIAM H. MCNEILL. *Venice: The Hinge of Europe, 1081–1797*. By Stanley Chojnacki 99
- SAMUEL ELIOT MORISON. *The European Discovery of America: The Southern Voyages, A.D. 1492–1616*.
By David B. Quinn 100
- YEHUDA ELKANA. *The Discovery of the Conservation of Energy*. By Harold Issadore Sharlin 101
- JACQUES DROZ, ed. *Histoire générale du socialisme*. Vol. 2. By Carl Landauer 101
- FRIEDRICH ENGEL-JANOSI et al., eds. *Denken über Geschichte: Aufsätze zur heutigen Situation des geschichtlichen Bewusstseins und der Geschichtswissenschaft*; HEINZ ANGERMEIER. *Geschichte oder Gegenwart: Reflexionen über des Verhältnis von Zeit und Geist*. By Georg G. Iggers 102
- E. D. PHILLIPS. *Aspects of Greek Medicine*.
By Wesley D. Smith 104
- ROBERT DREWS. *The Greek Accounts of Eastern History*.
By John J. Keaney 104
- PHILIPPE GAUTHIER. *Symbola: Les étrangers et la justice dans les cités grecques*. By Charles D. Hamilton 105
- ALAN WARDMAN. *Plutarch's Lives*.
By Thomas W. Africa 105
- G. V. SUMNER. *The Orators in Cicero's Brutus: Prosopography and Chronology*. By Glanville Downey 106
- MICHAEL GRANT. *The Army of the Caesars*.
By Michael P. Speidel 106
- GÉZA ALFÖLDY. *Noricum*. Tr. by ANTHONY BIRLEY.
By Henry C. Boren 107
- ALBINO GARZETTI. *From Tiberius to the Antonines: A History of the Roman Empire, AD 14–192*. Tr. by J. R. FOSTER.
By Mason Hammond 107

MEDIEVAL

- RUSSELL FRASER. *The Dark Ages & the Age of Gold*.
By John E. Wrigley 108
- VACLAV MUDROCH and G. S. COUSE, eds. *Essays on the Reconstruction of Medieval History*. By Francis Oakley 109

- JACQUES HEERS. *Le clan familial au Moyen Age: Étude sur les structures politiques et sociales des milieux urbains.*
By Joseph R. Strayer 109
- CHRISTOPHER BROOKE. *The Monastic World, 1000-1300.*
By Joseph Dahmus 110
- R. C. SMAIL. *The Crusaders: In Syria and the Holy Land.*
By Marshall W. Baldwin 110
- W. J. FRANK DAVIES. *Teaching Reading in Early England.*
By J. D. A. Ogilvy 111
- MICHAEL R. POWICKE. *The Community of the Realm.*
By Bryce Lyon 111
- ARTHUR STEPHEN MCGRADY. *The Political Thought of William of Ockham: Personal and Institutional Principles.*
By Bennett D. Hill 112
- ALBERT BOURGEOIS. *Lèpreux et maladreries du Pas-de-Calais (X^e-XVIII^e siècles): Psychologie collective et institutions charitables.* By R. C. Howard 113
- WALTER L. WAKEFIELD. *Heresy, Crusade and Inquisition in Southern France, 1100-1250.* By J. N. Hillgarth 113
- HELMUT LIPPELT. *Thietmar von Merseburg: Reichsbischof und Chronist.* By Janos M. Bak 114
- DAGMAR UNVERHAU. *Approbatio—Reprobatio: Studien zum päpstlichen Mitspracherecht bei Kaiserkrönung und Königswahl vom Investiturstreit bis zum ersten Prozess Johanns XXII. gegen Ludwig IV.* By Schafer Williams 114
- WALTER HÜFLECHNER. *Die Gesandten der europäischen Mächte, vornehmlich des Kaisers und des Reiches, 1490-1500.*
By Gunther E. Rothenberg 115
- PIERRE TOUBERT. *Les structures du Latium médiéval: Le Latium méridional et la Sabine du IX^e siècle à la fin du XII^e siècle.*
In 2 vols. By David Herlihy 115
- JOHN HOLLAND SMITH. *Francis of Assisi.*
By Charles T. Davis 116
- EUGENE I. COX. *The Eagles of Savoy: The House of Savoy in Thirteenth-Century Europe.* By William M. Bowsky 117
- WALTER BRANDMÜLLER. *Das Konzil von Pavia-Siena, 1423-1424.* Vols. 1, 2. By Brian Tierney 118
- M. N. TIKHOMIROV. *Rossiiskoe gosudarstvo XV-XVII vekov* [The Russian State during the 15th-17th Centuries];
N. N. POKROVSKII. *Aktovye istochniki po istorii chernosozhnogo zemlevladieniia v Rossii XIV-nachala XVI v.* [Documentary Sources on the History of Taxable State Ownership of Land in Russia from the 14th to the Beginning of the 16th Century]. Ed. by M. N. TIKHOMIROV. By Gustave Alef 118
- JAROSLAV PELIKAN. *The Christian Tradition: A History of the Development of Doctrine.* Vol. 2, *The Spirit of Eastern Christendom (600-1700).* By John E. Rexine 119
- Dumbarton Oaks Papers.* No. 27. By Robin Cormack 120
- MODERN EUROPE
- JOHN R. GILLIS. *Youth and History: Tradition and Change in European Age Relations, 1770-Present.*
By Tamara K. Hareven 121
- WILLIAM E. WILKIE. *The Cardinal Protectors of England: Rome and the Tudors before the Reformation.*
By Bruce P. Flood, Jr. 122
- O. V. ORLIK. *Peredovaiia Rossiia i revoliutsionnaia Frantsiia (I polovina XIX v.)* [Progressive Russia and Revolutionary France in the First Half of the 19th Century].
By Robert F. Byrnes 123
- ROGER BULLEN. *Palmerston, Guizot and the Collapse of the Entente Cordiale.* By Lawrence C. Jennings 123
- DAVID CHILDS. *Marx and the Marxists: An Outline of Practice and Theory;* V. F. PUSTARNAKOV. *"Kapital" K. Marksa i filosofskaia mysl' v Rossii* (Konets XIX-nachalo XX v.) [K. Marx's "Capital" and Philosophical Thought in Russia (End of the 19th-Beginning of the 20th Century)];
RICHARD N. HUNT. *The Political Ideas of Marx and Engels.* Vol. 1, *Marxism and Totalitarian Democracy, 1818-1850.*
By Paul Avrich 124
- ISTVÁN DIÓSZEGI. *Österreich-Ungarn und der französisch-preussische Krieg, 1870-1871.* By Louis A. Gebhard 125
- DWIGHT E. LEE. *Europe's Crucial Years: The Diplomatic Background of World War I, 1902-1914.*
By Gerard E. Silberstein 125
- A. A. SOLOV'EV et al., eds. *Russkie internacionalisty v bor'be za Vengerskuiu Sovetskuiu Respubliku 1919 g.: Sbornik dokumentov* [Russian Internationalists in the Struggle for the Hungarian Soviet Republic, 1919: A Collection of Documents]. By Ivan Avakumovic 126
- FRANÇOIS CROUZET. *Le conflit de Chypre, 1946-1959.* In 2 vols.;
STEPHEN G. NYDIS. *Cyprus: Reluctant Republic.*
By René Albrecht-Carrié 126
- MERVYN JAMES. *Family, Lineage, and Civil Society: A Study of Society, Politics, and Mentality in the Durham Region, 1500-1640.* By Roger Howell, Jr. 127
- MARTIN FLEISHER. *Radical Reform and Political Persuasion in the Life and Writings of Thomas More.* By J. H. Hexter 128
- A. HASSELL SMITH. *County and Court: Government and Politics in Norfolk, 1558-1603.* By G. L. Owens 128
- W. SPEED HILL, ed. *Studies in Richard Hooker: Essays Preliminary to an Edition of His Works.* By Leland H. Carlson 129
- COLIN G. C. TITE. *Impeachment and Parliamentary Judicature in Early Stuart England.* By Charles M. Gray 130
- J. S. MORRILL. *Cheshire 1630-1660: County Government and Society during the English Revolution.*
By Wallace T. MacCaffrey 130
- BRIAN MANNING, ed. *Politics, Religion and the English Civil War.* By Caroline Robbins 131
- SIR NEIL CANTLIE. *A History of the Army Medical Department.*
In 2 vols. By J. O. Baylen 132
- GRAHAM MIDGLEY. *The Life of Orator Henley.*
By Bertram H. Davis 132
- HERBERT M. ATHERTON. *Political Prints in the Age of Hogarth: A Study of the Ideographic Representation of Politics.*
By Howard H. Schless 133
- W. S. LEWIS et al., eds. *Horace Walpole's Correspondence with Henry Seymour Conway, Lady Ailesbury, Lord and Lady Hertford, Mrs Harris; Horace Walpole's Correspondence with Henry Seymour Conway, Lady Ailesbury, Lord and Lady Hertford, Lord Beauchamp, Henrietta Seymour Conway; Henry Walpole's Correspondence with Henry Seymour Conway, Lady Ailesbury, Lord and Lady Hertford, Lord Beauchamp, Lord Henry and Lord Hugh Seymour.* By Dora Mae Clark 133
- F. O. SHYLLON. *Black Slaves in Britain.*
By Robin W. Winks 134
- JOHN NELSON TARN. *Five per Cent Philanthropy: An Account of Housing in Urban Areas between 1840 and 1914;* ANTHONY

- SUTCLIFFE, ed. *Multi-Storey Living: The British Working-Class Experience*; ENID GAULDIE. *Cruel Habitations: A History of Working-Class Housing, 1780-1918*. By Robert M. Gutchen 135
- PIERS MACKESY. *Statesmen at War: The Strategy of Overthrow, 1798-1799*. By Steven Ross 136
- CHRISTOPHER HOWARD. *Britain and the Casus Belli, 1822-1902: A Study of Britain's International Position from Canning to Salisbury*. By Richard A. Cosgrove 137
- EDITH F. HUKWITZ. *Politics and the Public Conscience: Slave Emancipation and the Abolitionist Movement in Britain*. By Alan H. Adamson 137
- JANET ROEBUCK. *The Making of Modern English Society from 1850*. By Standish Meacham 138
- B. A. ROZHKOV. *Angliiskoe rabochee dvizhenie, 1859-1864 gg.* [The English Workers' Movement, 1859-1864]. By Paul B. Johnson 138
- GERTRUDE HIMMELFARB. *On Liberty and Liberalism: The Case of John Stuart Mill*. By Alan Ryan 139
- D. A. LOW. *Lion Rampant: Essays in the Study of British Imperialism*; C. C. ELDRIDGE. *England's Mission: The Imperial Idea in the Age of Gladstone and Disraeli 1868-1880*; IAN M. DRUMMOND. *British Economic Policy and the Empire, 1919-1939*. By A. P. Thornton 140
- RICHARD D. WOLFF. *The Economics of Colonialism: Britain and Kenya, 1870-1930*. By Harvey G. Soff 141
- MICHAEL ROE. *Kenealy and the Tichborne Cause: A Study in Mid-Victorian Populism*. By Walter L. Arnstein 142
- GILLIAN SUTHERLAND. *Policy-Making in Elementary Education, 1870-1895*. By Frederick M. Schweitzer 142
- G. N. UZOIGWE. *Britain and the Conquest of Africa: The Age of Salisbury*. By Herbert Paul Meritt 143
- ALFRED F. HAVIGHURST. *Radical Journalist: H. W. Massingham (1860-1924)*. By P. F. Clarke 143
- KENNETH D. BROWN, ed. *Essays in Anti-Labour History: Responses to the Rise of Labour in Britain*. By Barbara Malament 144
- JOHN GOUGH. *The Plans of War: The General Staff and British Military Strategy c. 1900-1916*. By Paul Guinn 145
- J. M. WINTER. *Socialism and the Challenge of War: Ideas and Politics in Britain, 1912-18*. By Bernard Semmel 145
- ARTHUR J. MARDER. *From the Dardanelles to Oran: Studies of the Royal Navy in War and Peace, 1915-1940*. By Theodore Ropp 146
- PAOLA BRUNDU OLLA. *Le origini diplomatiche dell'accordo navale anglo-tedesco del giugno 1935*. By Alan Cassels 146
- SIR GEORGE CATLIN. *For God's Sake, Go! An Autobiography*. By Neal Wood 147
- ANTHONY SUTCLIFFE and ROGER SMITH. *Birmingham 1939-1970*. By E. P. Hennock 147
- G. W. S. BARROW, ed. *The Scottish Tradition: Essays in Honour of Ronald Gordon Cant*. By Ranald Nicholson 148
- MARK DILWORTH. *The Scots in Franconia: A Century of Monastic Life*. By Mack Walker 148
- DAVID STEVENSON. *The Scottish Revolution, 1637-1644: The Triumph of the Covenanters*. By Robert Paul Barnes 149
- BRENDAN BRADSHAW. *The Dissolution of the Religious Orders in Ireland under Henry VIII*. By W. J. Jones 150
- BRIAN FARRELL, ed. *The Irish Parliamentary Tradition*. By Hereward Senior 150
- E. D. STEELE. *Irish Land and British Politics: Tenant-Right and Nationality, 1865-1870*. By Lawrence J. McCaffrey 150
- DOMINIC DALY. *The Young Douglas Hyde: The Dawn of the Irish Revolution and Renaissance, 1874-1893*. By Giovanni Costigan 151
- I. D. MCFARLANE. *A Literary History of France: Renaissance France, 1470-1589*. By Davis Bitton 151
- PIERRE GOUBERT. *L'Ancien Régime*. Vol. 2, *Les pouvoirs*. By Orest Ranum 152
- ROLAND MOUSNIER. *Les institutions de la France sous la monarchie absolue, 1598-1789*. Vol. 1, *Société et État*. By William F. Church 153
- DOROTHY ANNE LIOT BACKER. *Precious Women*. By Elisabeth Israel's Perry 154
- A. J. KRAILSHEIMER. *Armand-Jean de Rancé, Abbot of La Trappe: His Influence in the Cloister and the World*. By Norman Ravitch 155
- RENÉ MOULINAS. *L'imprimerie, la librairie et la presse à Avignon au XVIII^e siècle*. By Raymond Birn 156
- KENNEDY F. ROCHE. *Rousseau: Stoic & Romantic*. By Judith N. Shklar 156
- JOHN CHARVET. *The Social Problem in the Philosophy of Rousseau*; BRONISŁAW BACZKO. *Rousseau: Solitude et communauté*. Tr. by CLAIRE BRENDHIEL-LAMBOUT. By Gerald J. Cavanaugh 157
- LESTER G. CROCKER. *Diderot's Chaotic Order: Approach to Synthesis*. By Victor G. Wexler 158
- EDWARD SHORTER and CHARLES TILLY. *Strikes in France, 1830-1968*. By Martin R. Waldman 159
- JAN KARL TANENBAUM. *General Maurice Sarrail, 1856-1929: The French Army and Left-Wing Politics*. By David B. Ralston 160
- GERALD H. MEAKER. *The Revolutionary Left in Spain, 1914-1923*. By Joan Connelly Ullman 160
- R. A. B. MYNORS and D. F. S. THOMSON, IFS. *The Correspondence of Erasmus*. Vol. 1. By James D. Tracy 161
- C. H. E. DE WIT. *De nederlandse revolutie van de achttiende eeuw, 1780-1787: Oligarchie en proletariaat* [The Revolution in the Netherlands in the Eighteenth Century, 1780-1787: Oligarchy and Proletariat]. By Herbert H. Rowen 161
- H. F. COHEN. *Om de vernieuwing van het socialisme: De politieke oriëntatie van de Nederlandse sociaal-democratie 1919-1930* [Toward the Renewal of Socialism: The Political Orientation of the Dutch Social-Democratic Party 1919-1930]. By F. Gunther Eyck 162
- TAGE KAASTED. *Konseilspraesident C. Th. Zahles dagbøger 1914-1917* [President of the Council C. Th. Zahle's Diary 1914-1917]. By Erik J. Friis 163
- ERIK BEUKEL. *Socialdemokratiet og stationeringsproblemet 1952-53: En sikkerhedspolitisk beslutning* [The Social Democrats and the Problem of the Stationing of Foreign Troops 1952-53: A Political Decision on Security]. By Francis J. Bowman 163

- PAUL M. HAYES. *Quisling: The Career and Political Ideas of Vidkun Quisling, 1887-1945*. By Heinz E. Ellersieck 164
- ERICH KAHLER. *The Germans*. Ed. by ROBERT and RITA KIMBER. By Michael Ermarth 164
- Biographisches Wörterbuch zur deutschen Geschichte*. 2d rev. ed. by KARL BOSL et al. Vol. 2, I-R. By Helen P. Liebel 165
- T. C. W. BLANNING. *Reform and Revolution in Mainz, 1743-1803*. By John F. Flynn 165
- RICHARD S. CROMWELL. *David Friedrich Strauss and His Place in Modern Thought*. By Gordon D. Drummond 166
- HELGA KROHN. *Die Juden in Hamburg: Die politische, soziale und kulturelle Entwicklung einer jüdischen Grossstadtgemeinde nach der Emanzipation, 1848-1918*. By Gordon R. Mork 166
- FRITZ FISCHER. *World Power or Decline: The Controversy over Germany's Aims in the First World War*. Tr. by LANCELOT L. FARRAR et al. By Hans W. Gatzke 167
- DANKWART GURATZSCH. *Macht durch Organisation: Die Grundlegung des Hugenbergischen Presseimperiums*. By Theodore S. Hamerow 167
- INGRID SCHULZE-BIDLINGMAIER, ed. *Die Kabinette Wirth I und II: 10. Mai 1921 bis 26. Oktober 1921; 26. Oktober 1921 bis 22. November 1922*. Vols. 1, 2; GÜNTER ABRAMOWSKI, ed. *Die Kabinette Marx I und II: 30. November 1923 bis 3. Juni 1924; 3. Juni 1924 bis 15. Januar 1925*. Vols. 1, 2. By H. A. Turner, Jr. 168
- Akten zur deutschen auswärtigen Politik, 1918-1945, aus dem Archiv des Auswärtigen Amts*. Ser. B: 1925-1933. Vols. 4-6. By Felix E. Hirsch 169
- DONALD M. MCKALE. *The Nazi Party Courts: Hitler's Management of Conflict in His Movement, 1921-1945*. By G. G. Field 170
- ILSE MAURER. *Reichsfinanzen und Grosse Koalition: Zur Geschichte des Reichskabinetts Müller (1928-1930)*. By Henry Cord Meyer 170
- GÜNTER PLUM. *Gesellschaftsstruktur und politisches Bewusstsein in einer katholischen Region, 1928-1933: Untersuchung am Beispiel des Regierungsbezirks Aachen*. By John S. Conway 171
- NORMAN RICH. *Hitler's War Aims*. Vols. 1, 2. By D. E. Emerson 172
- ADOLF BEER. *Die Finanzen Österreichs im XIX. Jahrhundert, nach archivalischen Quellen*. By Herman Freudenberger 173
- WILLIAM J. MCGRATH. *Dionysian Art and Populist Politics in Austria*. By Andrew G. Whiteside 173
- DIETHILD HARRINGTON-MÜLLER. *Der Fortschrittsklub im Abgeordnetenhaus des Österreichischen Reichsrats, 1873-1910*. By Henry G. Dittmar 174
- M. A. POLTAVSKIĬ. *Avstriiskii narod i anshlius, 1938 g.* [The Austrian People and the Anschluss, 1938]; GERHARD BOTZ. *Die Eingliederung Österreichs in das Deutsche Reich: Planung und Verwirklichung des politisch-administrativen Anschlusses (1938-1940)*. By Stanley B. Winters 174
- JEAN-FRANÇOIS BERGIER. *Naissance et croissance de la Suisse industrielle*. By Heinz K. Meier 175
- CLEMENTE FUSERO. *The Borgias*. Tr. by PETER GREEN; E. R. CHAMBERLIN. *The Fall of the House of Borgia*. By Eric Cochrane 175
- ANDRÉ ROCHON et al. *Les écrivains et le pouvoir en Italie à l'époque de la Renaissance: L'Arioste, Bernardo Dovizi da Bibbiena, Machiavel, l'Académie Florentine, Giambattista Giralaldi Cinthio*. By Avery Andrews 177
- RAFFAELE CIASCA. *Aspetti economici e sociali dell'Italia pre-unitaria: Saggi*. By Emiliana P. Noether 177
- MARCELLA PINCHERLE. *Moderatismo politico e riforma religiosa in Terenzio Mamiani*. By Raymond Grew 178
- RENATO TREVES. *La dottrina sansimoniana nel pensiero italiano del Risorgimento*. By Ronald S. Cunsolo 178
- TONI CERUTTI. *Antonio Gallenga: An Italian Writer in Victorian England*. By Derek Beales 179
- FRIEDRICH ENGEL-JANOSI. *Vom Chaos zur Katastrophe: Vatikanische Gespräche, 1918 bis 1938. Vornehmlich auf Grund der Berichte der österreichischen Gesandten beim Heiligen Stuhl*. By George O. Kent 180
- IRMGARD WILHARM. *Die Anfänge des griechischen Nationalstaates, 1833-1843*. By Frederick B. Chary 180
- HEINZ RICHTER. *Griechenland zwischen Revolution und Konterrevolution (1936-1946)*. By John C. Campbell 181
- M. BERZA et al., eds. *Relatii româno-bulgare de-a lungul veacurilor (Sec. XII-XIX): Studii* [Romanian-Bulgarian Relations in the Course of the Centuries (12th-19th Centuries): Studies]. By Stephen Fischer-Galați 181
- R. J. W. EVANS. *Rudolf II and His World: A Study in Intellectual History, 1576-1612*. By John P. Spielman 182
- JAROSLAW PELENSKI. *Russia and Kazan: Conquest and Imperial Ideology (1438-1560s)*. By D. C. Matuszewski 183
- M. I. SLUKHOVSKIĬ. *Russkaia biblioteka XVI-XVII vv.* [The Russian Library in the 16th and 17th Centuries]; S. P. LUPPOV. *Kniga v Rossii v pervoi chetverti XVIII veka* [The Book in Russia in the First Quarter of the 18th Century]. By C. Bickford O'Brien 183
- M. F. SHABAEVA, ed. *Ocherki istorii shkoly i pedagogicheskoi mysli narodov SSSR, XVIII v.-pervaia polovina XIX v.* [Essays on the History of Schools and Pedagogical Thought of the Peoples of the USSR, 18th-First Half of 19th Century]. By Edward C. Thaden 184
- V. IA. LAVERYCHEV. *Tsarizm i rabochii vopros v Rossii (1861-1917 gg.)* [Tsarism and the Labor Question in Russia (1861-1917)]. By Roland V. Layton, Jr. 185
- DAVID MACKENZIE. *The Lion of Tashkent: The Career of General M. G. Cherniaev*. By Thomas B. Rainey 185
- V. S. POZNANSKIĬ. *Ocherki istorii vooruzhennoi bor'by Sovetov Sibiri s kontrrevoliutsiei v 1917-1918 gg.* [Essays on the Armed Struggle of the Siberian Soviets against the Counterrevolution, 1917-1918]. By Canfield F. Smith 187
- ALFRED ERICH SENN. *Diplomacy and Revolution: The Soviet Mission to Switzerland, 1918*. By Rex A. Wade 187
- S. N. SEMANOV. *Likvidatsiia antisovetskogo kronshtadtskogo miatezha 1921 goda* [The Liquidation of the Anti-Soviet Kronstadt Revolt of 1921]; ROGER PETHYBRIDGE. *The Social Prelude to Stalinism*. By Alexander Dallin 188
- RONALD HINGLEY. *Joseph Stalin: Man and Legend*. By Stephan M. Horak 189
- Istoria mist i sil Ukraïns'koï RSR* [History of the Cities and Villages of the Ukrainian SSR]. *Kherson's'ka Oblast'* [Kherson Province], ed. by O. E. KAS'IANENKO et al.; *Kirovohrads'ka Oblast'* [Kirovograd Province], ed. by D. S. SYVOLAP et al.; *Rovens'ka Oblast'* [Rovno Province], ed. by

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By John A. Armstrong 189

NEAR EAST

- ELIAS SHOUFANI. *Al-Riddah and the Muslim Conquest of Arabia*. By Reuben W. Smith 190
- W. MONTGOMERY WATT. *The Majesty that Was Islam: The Islamic World, 961-1100*. By Wilson B. Bishai 191

AFRICA

- J. D. CLARK. *Kalambo Falls Prehistoric Site*. Vols. 1, 2. By Frank Willett 191
- KENNETH POST and MICHAEL VICKERS. *Structure and Conflict in Nigeria 1960-1966*. By Frank Chalk 192

ASIA AND THE EAST

- FRANK A. KIEMAN, JR., and JOHN K. FAIRBANK, eds. *Chinese Ways in Warfare*. By Romeyn Taylor 193
- ELLSWORTH C. CARLSON. *The Foochow Missionaries, 1847-1880*. By Paul A. Varg 193
- J. D. FRODSHAM, tr. *The First Chinese Embassy to the West: The Journals of Kuo Sung-t'ao, Liu Hsi-hung, and Chang Te-yi*. By Michael Gasster 194
- DAVID L. DAVIS *et al.* *Medieval Japan: Essays in Institutional History*. Ed. by JOHN W. HALL and JEFFREY P. MASS. By Peter Duus 194
- PHILIP MASON. *A Matter of Honour*. By John Tottenham 195
- BAWA SATINDER SINGH. *The Jammu Fox: A Biography of Maharaja Gulab Singh of Kashmir, 1792-1857*. By Neil Raitoy 196
- NICHOLAS MANSERGH, ed.-in-chief. *The Transfer of Power, 1942-7*. Vol. 4, *The Bengal Famine and the New Viceroyalty, 15 June 1943-31 August 1944*. By Robert Eric Frykenberg 196
- CHARLES A. JOINER. *The Politics of Massacre: Political Processes in South Vietnam*. By David Wurfel 197
- SARTONO KARTODIRDJO. *Protest Movements in Rural Java: A Study of Agrarian Unrest in the Nineteenth and Early Twentieth Centuries*. By Ruth T. McVey 198
- SIONE LÄTŪKEFU. *Church and State in Tonga: The Wesleyan Methodist Missionaries and Political Development, 1822-1875*. By D. R. SarDesai 199
- TOM GIBSON. *The Maori Wars: The British Army in New Zealand, 1840-1872*. By Peter J. Coleman 200

UNITED STATES

- FRANK FREIDEL, ed. *Harvard Guide to American History*. In 2 vols. Rev. ed.; JOHN A. GARRATY, ed. *Encyclopedia of American Biography*. By Walter Rundell, Jr. 200
- JEROME FINSTER, ed. *The National Archives and Urban Research*. By Constance McLaughlin Green 201
- American Narrative Painting*. Catalog notes by NANCY WALL MOURE. Essay by DONELSON F. HOOPES; MATTHEW BAIGELL.

- The American Scene: American Painting of the 1930's*; WILLIAM H. GERDYS. *The Great American Nude: A History in Art*. By Lillian B. Miller 202

- KENNETH A. LOCKRIDGE. *Literacy in Colonial New England: An Enquiry into the Social Context of Literacy in the Early Modern West*. By David Cressy 203

- PETER H. WOOD. *Black Majority: Negroes in Colonial South Carolina from 1650 through the Stono Rebellion*. By Robert McColley 204

- EDMUND BERKELEY and DOROTHY SMITH BERKELEY. *Dr. John Mitchell: The Man Who Made the Map of North America*. By Sam Hilliard 204

- CATHERINE DRINKER BOWEN. *The Most Dangerous Man in America: Scenes from the Life of Benjamin Franklin*. By Max Savelle 205

- PAUL K. CONKIN. *Self-Evident Truths: Being a Discourse on the Origins & Development of the First Principles of American Government—Popular Sovereignty, Natural Rights, and Balance & Separation of Powers*. By William M. Dabney 205

- RICHARD A. BARTLETT. *The New Country: A Social History of the American Frontier, 1776-1890*. By Robert V. Hine 206

- HAROLD C. SYRETT, ed. *The Papers of Alexander Hamilton*. Vols. 20, 21. By John Pancake 206

- DICKSON D. BRUCE, JR. *And They All Sang Hallelujah: Plain-Folk Camp-Meeting Religion, 1800-1845*. By John M. Mulder 207

- WALTER DONALD KRING. *Liberals among the Orthodox: Unitarian Beginnings in New York City, 1819-1839*. By Daniel Walker Howe 208

- ROBERT C. DICK. *Black Protest: Issues and Tactics*. By Benjamin Quarles 208

- EUGENE D. GENOVESE. *Roll, Jordan, Roll: The World the Slaves Made*. By Marion Kilson 209

- ROGER W. LOTCHIN. *San Francisco, 1846-1856: From Hamlet to City*. By Walter S. Gilazer 210

- JONATHAN KATZ. *Resistance at Christiana: The Fugitive Slave Rebellion, Christiana, Pennsylvania, September 11, 1851. A Documentary Account*. By Peter M. Mitchell 210

- DONALD R. WARREN. *To Enforce Education: A History of the Founding Years of the United States Office of Education*. By Theodore R.Sizer 211

- R. GORDON KELLY. *Mother Was a Lady: Self and Society in Selected American Children's Periodicals, 1865-1890*. By Bernard W. Wishy 211

- HANS-ULRICH WEHLER. *Der Aufstieg des amerikanischen Imperialismus: Studien zur Entwicklung des Imperium Americanum, 1865-1900*. By Lloyd E. Ambrosius 212

- ALAN HYNDING. *The Public Life of Eugene Semple: Promoter and Politician of the Pacific Northwest*. By Ted C. Hinckley 212

- JAMES EDWARD WRIGHT. *The Politics of Populism: Dissent in Colorado*. By Lewis L. Gould 213

- GERALD P. FOGARTY, S.J. *The Vatican and the Americanist Crisis: Denis J. O'Connell, American Agent in Rome, 1885-1903*. By Frank T. Reuter 213

- LOUIS R. HARLAN *et al.*, eds. *The Booker T. Washington Papers*. Vol. 3. By Robert G. Sherer 214

THOMAS S. HINES. <i>Burnham of Chicago: Architect and Planner</i> . By Kenneth T. Jackson 215	<i>the World, 1945-1973</i> . Tr. by FRANK JELLINEK. By Raymond A. Esthus 220
J. NOEL HEERMANCE. <i>Charles W. Chesnutt, America's First Great Black Novelist</i> . By Frances Richardson Keller 215	<i>Foreign Relations of the United States, 1948</i> . Vol. 4, <i>Eastern Europe; The Soviet Union</i> . By W. W. Kulski 221
THOMAS B. TURNER. <i>Heritage of Excellence: The Johns Hop- kins Medical Institutions, 1914-1947</i> . By Saul Benison 216	IRWIN UNGER. <i>The Movement: A History of the American New Left, 1959-1972</i> . By Bernard Sternsher 222
MIRA WILKINS. <i>The Maturing of Multinational Enterprise: American Business Abroad from 1914 to 1970</i> . By Louis Galambos 216	<i>Public Papers of the Presidents of the United States. Richard Nixon. Containing the Public Messages, Speeches, and Statements of the President</i> . [Vol. 4.] 1972. By William C. Berman 222
ZOSA SZAJKOWSKI. <i>Jews, Wars, and Communism</i> . Vol. 2, <i>The Impact of the 1919-20 Red Scare on American Jewish Life</i> . By David W. Levy 217	CANADA
THOMAS B. BUELL. <i>The Quiet Warrior: A Biography of Admi- ral Raymond A. Spruance</i> . By Philip A. Crowl 217	H. V. NELLIS. <i>The Politics of Development: Forest, Mines & Hydro-Electric Power in Ontario, 1849-1941</i> . By Margaret Prang 223
MARTIN BLUMENSON. <i>The Patton Papers: 1940-1945</i> . By John K. Mahon 218	GENEVIÈVE LAIOUX-JAIN. <i>Les manuels d'histoire du Canada au Québec et en Ontario (de 1867 à 1914)</i> . By Cameron Nish 224
H. ESSAME. <i>Patton: A Study in Command</i> ; CHARLES B. MAC- DONALD. <i>The Last Offensive</i> . By Dello G. Dayton 219	TERRY COPP. <i>The Anatomy of Poverty: The Condition of the Working Class in Montreal 1897-1929</i> . By G. S. French 224
ROBERT A. GARSON. <i>The Democratic Party and the Politics of Sectionalism, 1941-1948</i> . By Alonzo L. Hamby 219	ANDRÉ-J. BÉLANGER. <i>L'apolitisme des idéologies québécoises: Le grand tournant de 1934-1936</i> . By Joseph A. Boudreau 225
EDWARD W. CHESTER. <i>Clash of Titans: Africa and U.S. For- eign Policy</i> . By Harold G. Marcus 220	LATIN AMERICA
LYNN H. MILLER and RONALD W. PRUESSEN, eds. <i>Reflections on the Cold War: A Quarter Century of American Foreign Policy</i> ; RAYMOND ARON. <i>The Imperial Republic: The United States and</i>	FREDERICK P. BOWSER. <i>The African Slave in Colonial Peru, 1524-1650</i> . By James Lockhart 225
	DONALD J. MABRY. <i>Mexico's Acción Nacional: A Catholic Alternative to Revolution</i> . By William Dirk Raat 226
Communications 227	Other Books Received 233
Recent Deaths 230	Index of Advertisers 60(a)

History as a Moral Science

GORDON WRIGHT

SOME YEARS AGO, a young American historian undertook the somewhat ghoulish task of unearthing all of the presidential addresses delivered before the American Historical Association during the first fifty years of its existence.¹ His purpose, in addition to earning the doctorate, was to discover the kinds of issues that confronted the profession during that half century—or, at least, the issues that presidents of the association thought to be of central importance. Occasionally these addresses created a mild stir of interest, and even some discussion; a few endured to be read and reread by later generations, as milestones or monuments of the historiographical enterprise. More commonly, they inspired such comments as that of Carl Becker shortly after the 1936 convention: “I went to Providence for the meeting of the A.H.A.—a terrible crush of about 1000 registrations: difficult to see anyone except by accident. . . . McIlwain didn’t do himself justice in the Pres. Ad.: but then very few do.”²

The history of this particular ritual thus offers at least one warning: those who practice it might do well not to take their pronouncements as the voice of God or the crystallized wisdom of the ages. It may be significant that AHA custom calls on the president to speak *ex cathedra* not at the outset of his term of office but at the very end, only forty-eight hours before he “passes into history,” as the saying goes. By that time it is much too late for him to make promises, to influence the association’s future course, or even to be held to answer for his stewardship or for such sophistries as his swan song may contain. He is allowed one Parthian shot—a gesture that no doubt has symbolic value, but that wins few battles, and rarely creates enduring legends.

How, then, should one perform this ritual most expeditiously and constructively? Seeking guidance from past practice, I find that presidential addresses seem to fall into one of three broad categories: those that reminisce, in the fashion of “forty years on”; those that embody the fruits of specialized scholarship; and those that seek to advance a cause or convey a message. The first sort possesses a nostalgic charm and provides the audience with a

¹ Herman Ausubel, *Historians and Their Craft* (New York, 1950).

² Becker to Leo and Ida Gershoy, Feb. 7, 1937, in “What Is the Good of History?” *Selected Letters of Carl L. Becker, 1900–1945*, ed. Michael Kammen (Ithaca, 1973), 252.

tranquil evening. The second, much favored over the years, takes the form of a learned essay focused on the speaker's own special field, offered as a gemlike miniature, a model of the mature research and reflection to which we all aspire, a tribute to the seriousness of learning. When performed with mastery, such a presentation may inspire historians far beyond the limited field of the speaker himself. It has the further merit of insulating the speaker against serious criticism and thus ensuring him a quiet departure from office, undisturbed by ironic witticisms or cries of outrage.

The third variety of presidential pronouncement involves more serious risks. It seeks to identify a broad issue that cuts across many fields of history, that relates to what most of us do in our professional capacity, and that either opens a new debate or, more commonly, reopens an old one. It poses a question that has been, either consciously or subconsciously, nagging at the speaker's psyche and at those of at least some of his fellow historians. Its tone can range from the calmly reflective through the confidently prescriptive to the downright preachy. It may stir up a storm; it may fade quietly into the night, like any puff of hot air. The record shows that the association has seen and heard a considerable variety of manifestoes of this sort.

A title such as "History as a Moral Science" clearly belongs in the "message" category, and probably in its more preachful subdivision. Furthermore, it may seem almost recklessly provocative. The idea of consciously reintroducing the moral dimension into history runs counter to the basic training of most historians, and probably to their professional instinct as well. Each of us has some strong views on the general subject of morality; each of us knows the dangers involved in making moral judgments in our work, or even suggesting a need for them. Worse still, a phrase like "moral science" has both a paradoxical and an anachronistic ring; it evokes the Victorian era, the times of Mill, Emerson, and Acton, when life was real and earnest and when coupling the words "moral" and "science" did not yet seem a case of illegal miscegenation. True, the phrase has survived in vestigial form into our own day: witness the French and the Belgian Academies of Moral and Political Sciences, which include historians within their ranks—though whether they belong to the political or moral branch is not entirely clear. But even the members of those academies, I suspect, would no longer try to argue very vigorously for resurrecting the phrase "moral science"—unless they happen to have a taste for the archaic.

ALTHOUGH PRESIDENTS OF THIS ASSOCIATION have often prescribed or preached from this podium, it has been a long time since one ventured rashly into the swamps of moral and value judgments. Henry Charles Lea did it in 1903, entitling his much-quoted address "Ethical Values in History"³—and prudently absenting himself from the convention while the corresponding

³ Henry Charles Lea, "Ethical Values in History," *AHR*, 9 (1903-04): 233-46.

secretary read out his speech. Lea's message, however, was not a defense but a denunciation of the thesis that the historian should make moral judgments. His target was that *giant terrible*, Lord Acton, who had just proclaimed the historian's duty to "suffer no man and no cause to escape the undying penalty which history has the power to inflict on wrong."⁴ Acton had excoriated those historians who gloss over crimes of past eras: "The strong man with the dagger," he declared, "is followed by the weak man with the sponge."⁵ Acton was dreadfully wrong, said Lea; moral standards change from one epoch to another; though we may sometimes feel righteous indignation, we must "strenuously repress it as a luxury to be left to [the] reader"; we must not write history "as a Sunday-school tale for children of larger growth."⁶ Lea's audience was receptive; after all, he was only saying what the standard manuals of historical method had already begun to assert as the orthodoxy of a scientific age. Lea's immediate successor, Goldwin Smith (an Actonian) entered a mild objection, suggesting that Lea's stance would, as Smith put it, "destroy the identity of the moral law";⁷ but this was only a glancing and ineffective blow, and the issue virtually disappeared from later presidential addresses—as it did also from manuals of historical method thereafter. There was a curious interlude in 1923 when Edward P. Cheyney told the association that in his search for laws of history (the historian's true function, he declared), he had detected one called the "law of moral progress"; he added, as a kind of rider, the curious dictum that "the people" are "always more moral than their rulers."⁸ And in 1949 Conyers Read returned to the problem in a considerably different context. He spoke at the height of the cold war and in a crusading spirit. He declared that the time was past for neutrality. "Total war . . . enlists everyone"; historians, like all others, must be mobilized in defense of our society's standards and ideals. Certain fundamental values must be recognized as beyond dispute. "This sounds like the advocacy of one form of social control as against another. In short, it is. But I see no alternative in a divided world. . . . The important thing is that we shall accept and endorse such controls as are essential for the preservation of our way of life." True, he declared that "this need not imply any deliberate distortion of the past"; but, he added, we must remember that not everything we learn about the past "is appropriate for broadcasting at street corners."⁹ Perhaps it is not surprising that during the quarter century since Read's time, presidents of this association have once again given a wide berth to issues of morality and values in history.¹⁰

⁴ Lord Acton, "Inaugural Lecture on the Study of History," in *Essays in the Liberal Interpretation of History*, ed. William H. McNeill (Chicago, 1967), 351.

⁵ Quoted in G. M. Trevelyan, "Bias in History," *History*, 32 (1947): 12.

⁶ Lea, "Ethical Values in History," 237.

⁷ Goldwin Smith, "The Treatment of History," *AHR*, 10 (1904-05): 513.

⁸ Edward P. Cheyney, "Laws in History," *AHR*, 29 (1923-24): 244.

⁹ Conyers Read, "Social Responsibilities of the Historian," *AHR*, 55 (1949-50): 283-84. Read's blunt remarks continue to haunt his memory; the most recent evidence is a Soviet report presented at the Fourteenth International Congress of Historical Sciences in San Francisco, August 1975: A. T. Danilov *et al.*, "History and Society," 44.

¹⁰ Louis Gottschalk and Dexter Perkins might be considered partial exceptions. Gottschalk barely

But if official pronouncements, as well as orthodox manuals, have largely been silent, some individual historians in recent years have felt the impulse to re-examine the problem—either because it presents a persistent intellectual challenge, or because their teaching function forces them to confront it.¹¹ Neither our audience nor the condition of the world in which we live any longer allows us the luxury of escape into a Proustian cork-lined ivory tower free of dust, microbes, and values. Those historians who have grappled with the subject in print are most often scholars of philosophic or methodological bent. But for the rest of us, especially in our classroom role, the theoretical debate—basic though it is—may be less crucial than the practical dilemmas forced upon us in a time of ideological conflict and intense moral ambiguity. No doubt those of us who profess contemporary history have found the dilemma sharpest; whoever must deal with the more brutal aspects of the Hitler or Stalin era, or with the devastating mass impact of mechanized total war, finds it hard to restrain some expression of that righteous indignation outlawed by Henry Charles Lea. But it is not only the contemporary historian who may feel a twinge of self-doubt about his educational role at this point in time. The issue was stated most bluntly at the height of the Watergate

touched on the inescapability of moral judgments by the historian. "A Professor of History in a Quandary," *AHR*, 59 (1953-54): 277-78. Perkins, in the only presidential address focused entirely on the historian as teacher, argued briefly but cogently that "we need not be afraid to speak of moral values," and urged attention by teachers to "the majestic example set by some of the great figures of our history, or all history." "We Shall Gladly Teach," *AHR*, 62 (1956-57): 309, 302.

¹¹ Among the recent attempts by American historians to grapple with this problem, I have found John Higham's essay, "Beyond Consensus: The Historian as Moral Critic," especially congenial and provocative. *AHR*, 67 (1961-62): 609-25. Other stimulating treatments include Henry Steele Commager, "Should the Historian Sit in Judgment?" in his *The Search for a Usable Past* (New York, 1967); T. P. Donovan, *Historical Thought in America* (Norman, 1973); David Hackett Fischer, *Historians' Fallacies* (New York, 1970); J. H. Hexter, *Doing History* (Bloomington, 1971); Edward A. Purcell, Jr., *The Crisis of Democratic Theory* (Lexington, 1973); Page Smith, *The Historian and History* (New York, 1964); Hugh Stretton, *The Political Sciences* (New York, 1969); P. E. Tillinghast, *The Specious Past* (Reading, Mass., 1972); Gene Wise, *American Historical Explanations* (Homewood, Ill., 1973); Howard Zinn, *The Politics of History* (Boston, 1970). In Britain, the debate has gone on sporadically ever since the famous controversy between Mandell Creighton and Acton, which constitutes a kind of *locus classicus*. The most recent phase was inaugurated by Herbert Butterfield's philippic against "Moral Judgments in History" in his *History and Human Relations* (New York, 1952), answered by the philosopher Isaiah Berlin in *Historical Inevitability* (London, 1955). Other voices include those of Geoffrey Barraclough, *History in a Changing World* (Oxford, 1957); C. V. Wedgwood, *Truth and Opinion: Historical Essays* (London, 1960); David Knowles, *The Historian and Character* (Cambridge, 1963); E. H. Carr, *What Is History?* (New York, 1963); and G. R. Elton, *The Practice of History* (London, 1969). Professional philosophers have written extensively on the more technical aspects of the problem and have been inclined, as William H. Dray points out, to ask different questions. Historians, he remarks, usually proceed in a quasi-psychological manner, debating whether man's nature will permit value neutrality in practice. Philosophers, on the other hand, want to know whether value judgments "enter into the very structure of historical inquiry." For an excellent brief summary and bibliography, see Dray, "History and Value Judgments," in Paul Edwards, ed., *Encyclopedia of Philosophy* (New York, 1967), 4: 26-30; Dray's pamphlet *Philosophy of History* (Englewood Cliffs, 1964) contains a somewhat fuller version. Two recent and provocative philosophical treatments are G. R. Grice, *The Grounds of Moral Judgment* (Cambridge, 1967), and Richard M. Hare, *Applications of Moral Philosophy* (London, 1972). Grice attempts to distinguish between two kinds of obligation: "basic" (the essentially legalistic sector of morality) and "ultra" (the ethical part of morality, beyond the legal minimum). For questions of the former sort, he argues, professional "moral scientists" are required; the second sort, which involve human character and must be settled by insight and reflection, are not susceptible to scientific analysis and are properly in the province of nonscientific types like novelists—and, presumably, historians. The Fourteenth International Congress of Historical Sciences at San Francisco gave a featured place to a session on "Value Reference and Value Judgments in Historiography," with the principal report presented by A. G. Weiler of the Netherlands.

melodrama by the eminent literary critic Henri Peyre: "Those of us who have been entrusted with the education of the young may well ask ourselves a harrowing question: Have we failed lamentably to impart any moral sense, any critical spirit to those whom we have instructed?" Those involved in Watergate and related escapades, Peyre pointed out, were not slum-born *mafiosi* but men who have enjoyed the most advanced educational opportunities offered by our society; more than most others, they should have had a sophisticated grasp of basic values and should have been clearly aware of their moral responsibilities.¹² Yet these highly privileged public officials apparently emerged from our universities as moral illiterates or astigmatics, and even after their disgrace they often appeared bewildered or angry rather than chastened and contrite. Thus one former presidential adviser, after the White House roof had fallen in, accused the public and the media of having set moral standards for public servants that are "really mythical," and clinched his case by asking rhetorically, "Would you rather have a competent scoundrel or an honest boob in office?"¹³

FOR US AS HISTORIANS the question is whether we as professionals bear any responsibility for bringing on such a state of affairs, and, as a corollary, whether we are obligated to do anything about it. Facing up to such questions involves a venture into a kind of no man's land, liberally strewn with booby traps and dead bodies. I take some comfort, however, in recalling that at least one other historian once found himself in a similar predicament. Carl Becker wrote to a friend in the summer of 1931: "Now I will have to get to work again: . . . the completion of the blasted presidential address. I had it 2/3 finished in June, and then it stuck. I don't know how to end it."¹⁴ But in the end, end it he did; and so must his successors, even if less triumphantly.

Surely few of us here, and few of our critics either, would hold the historians solely or principally to blame for the moral transgressions of certain public officials, or for the more pervasive ethical confusion that seems to suffuse our age. The fact that some unscrupulous men have made their way into high places and have misused their power is of course not new; nor does it necessarily mean that we historians helped put them there, or that we could somehow have prevented their misdeeds. We might therefore choose to disclaim all responsibility, charging the fault to those in our society who profess to be its moral guides, or to the obtuseness of our former students who failed to penetrate the message hidden somewhere in our unbiased teaching. But that—as a fellow Californian liked to say—would be the easy way. Whether we enjoy it or not, we must ask ourselves whether we bear a more diffused kind of responsibility. Some critics assert that we historians, by insisting over the years that moral standards are relative across cultures and over time, have

¹² Henri Peyre, letter to the editor, *New York Times*, May 30, 1973.

¹³ J. Fred Buzhardt, quoted in the *New York Times*, Mar. 12, 1975.

¹⁴ Becker to Gershey [summer 1931], in "*What Is the Good of History?*" 145.

seriously undermined our own capacities—and a fortiori those of our students—to make moral judgments of any kind. In eschewing the horrors of moral rhetoric, they say, we have drifted into a moral vacuum; to avoid the charge of moral self-righteousness, we have preferred simply to abdicate. True, we have clothed our conduct in attractive garb: we speak of detachment, open-mindedness, tolerance, understanding. But beneath these euphemisms, the critics say, abdication is the essential reality. Twenty years ago our regretted colleague Raymond Sontag was already warning us about this trend: “We historians,” he wrote, “have worked so hard to eliminate passion and fanaticism from our thinking, that we have forgotten how to describe a way of life dominated by passion and fanaticism, and actions which are evil.”¹⁵ And C. V. Wedgwood added a further admonition: “History dispassionately recorded,” she declared, “nearly always sounds harsh and cynical. History is not a moral tale, and the effect of telling it without comment is, inevitably, to underline its worst features: the defeat of the weak by the strong, the degeneration of ideals, the corruption of institutions, the triumph of intelligent self-interest.”¹⁶ History thus presented, she warned, was likely to produce a mood of cynicism among those exposed to it—a mood that might well suggest that political leaders can only be either competent scoundrels or honest boobs. It might even suggest that Leo Durocher’s law about nice guys finishing last has some kind of universal and timeless validity.

For a long time, of course, historians comforted themselves with the thought that dispassionate value-free history would somehow secrete its own moral lessons, or would at least ensure that those who study it would be led somewhat automatically to sensible and judicious conclusions. Thus Henry Charles Lea, after delivering his thunderbolts against Acton, could conclude that history “may and it generally will, convey a moral, but that moral should educe itself from the facts.”¹⁷ Most of us today are a bit less sanguine about the automatic nature of the process, yet the idea does persist that the path from raw data to sophisticated judgment needs no guideposts along the route. And even when we are not so sure that the process is easy or automatic, the alternative—guideposts suggested by the historian, functioning as a moral critic as well as a purveyor of facts—continues to be seen as either illegitimate or ineffective. Thus Henry Steele Commager, after a thoughtful look at both horns of the dilemma, concludes that moral judgments are both “arrogant” and “futile,” and he denies that readers need the historian’s “moral instruction”; while the Belgian scholar Jean Stengers warns that even when the historian’s moral judgments are solidly founded upon a thorough and dispassionate study of the evidence, they are likely to undermine his purpose because his audience will suspect him of grinding an ax. The historian’s best hope of being morally effective, Stengers concludes, is to provide a living

¹⁵ Raymond J. Sontag, “The Democracies and the Dictators since 1933,” *Proceedings of the American Philosophical Society*, 98 (Philadelphia, 1954): 317.

¹⁶ Wedgwood, *Truth and Opinion*, 52.

¹⁷ Lea, “Ethical Values in History,” 237.

example of respect for the one “fragile” value that transcends all others—absolute respect for the truth.¹⁸ The case he makes is a remarkably compelling one, yet somehow it leaves one vaguely unsatisfied.

This recent process of soul searching has been confined mainly, I suspect, to those of us who find our identity somewhere in the so-called liberal tradition, broadly defined. Our conservative colleagues—at least those who are self-consciously conservative—have had it easier; a good many of them have always been quite openly committed to a system of absolute values, religiously or ethically based, by which the events of the past can be confidently judged without the least embarrassment. On the left, many self-styled radical historians have vigorously asserted the idea of a committed history, either because they too possess a coherent *Weltanschauung* with something like its own absolutes, or because they view history instrumentally, as a tool to achieve social and political change. Both the conservative and the radical positions obviously have their legitimate place in the educational process, so long as neither is imposed as unchallengeable dogma. But the liberals among us—even the “closet liberals” who are reluctant to bear the stigma of a shopworn label—continue to be haunted by our pluralistic, skeptical, antidogmatic heritage, our rejection of absolutes, our distaste for anything that might look like indoctrination. The result, it seems to me, is that while many students of history may be exposed these days to vigorous and confident expositors of either the conservative or the radical value system, they rarely receive any clear vision of the past as it appears in the light of liberal values. I am sometimes tempted to think that we liberals have been re-enacting the charge of the Light Brigade: while cannon volley and thunder to the right and to the left of us, we ourselves gallop on in a cloud of dust, unsure just which way is forward, and shouting to those who follow us to study the map and draw their own conclusions. If part of an educator’s responsibility is to offer some sort of positive guidance, then perhaps it is true that many of us have unthinkingly abdicated.

True, there are critics who doubt that the abdication has been real—that we have actually practiced the dispassionate objectivity that we preach. “Our smartest radicals,” remarked Carl Becker more than forty years ago, “suspect [the liberal] of being an *agent provocateur* [*sic*] of Capitalism, while conservatives of ancient lineage treat [him] as a Bolshevik masquerading in a rented dress suit.”¹⁹ The leading spokesman of the New Left detects beneath the sham of open-mindedness a hidden vice: support of “the Existing Institution.”²⁰ A talented young American historian, arguing the case of the *Annales* school, charges that “American historical writing still largely consists of parochial and moralistic studies of events, policies, and individuals,” and

¹⁸ Commager, *Search for a Usable Past*, 300–22; Jean Stengers, “Quelques réflexions sur le jugement moral en histoire,” Académie Royale de Belgique, *Bulletin de la Classe des Lettres et des Sciences Morales et Politiques*, 5th ser., 58 (1972–73): 189–203.

¹⁹ Carl L. Becker, “The Dilemma of Liberals in Our Time,” in *Detachment and the Writing of History: Essays and Letters of Carl L. Becker*, ed. Phil L. Snyder (Ithaca, 1958), 188.

²⁰ William Appleman Williams, *History as a Way of Learning* (New York, 1973), 162.

proceeds to consign both liberals and Marxists to oblivion as exponents of a dead morality.²¹ We liberals thus stand accused in various quarters of violating our own professed standards, and acting in reality (to borrow Heinz Hartmann's phrase) as "hidden preachers."²²

Whether our real fault is hypocrisy or abdication, those of us in the liberal tradition—a sizable remnant still, though probably an endangered species—feel most keenly the whiplash of this particular dilemma. Not all of us will be ready to change our ways—to risk giving up our accustomed armor, that somewhat gray and aloof neutrality (or costume of neutrality) that has been so comfortably protective. Some of us will not find it easy to abandon our indulgent fascination for the charismatic rascals and the melodramatic episodes in history—an indulgence that adds some more vivid colors to the basic gray, thus enlivening our prose and awakening our drowsy students. Nor will all of us readily shake free from the temptation to fix cynically on the flaws and foibles of every leader, nation, age, or professed ideal, to the point that the very words "moral" and "value" take on ironic overtones. Furthermore, it would be self-defeating if all liberal historians were to forswear the ideal of dispassionate *Wertfreiheit*, for out of that tradition have come—and will doubtlessly continue to come—some of the most impressive products of our profession.

Nevertheless, I believe that a case can be made for relegitimizing the writing and teaching of history by liberals whose model is neither the neutral scientist nor the "hidden preacher" but the exponent of a self-conscious and coherent value system. If one purpose of historical study is to broaden and enrich the minds of students so that they can shape their own values and arrive at their own judgments (as I think they should), that purpose is likely to be best served if they are offered not only raw data and quantified facts, but also broad exposure to various mature interpretations of the past. The liberal interpretation belongs in that spectrum: indeed, perhaps more so today than ever before. In an age of unprecedented complexity, when ideological fanaticism, sporadic bursts of tribal fury, and the advocacy of "realism" in both its crude and its sophisticated form put world stability and even human existence at risk, the liberal temper may offer the nearest thing to a set of guideposts through the mine field. Its rejection of a black-and-white world in which the battalions of good and evil line up in serried ranks; its awareness of ambiguity as a profound and pervasive presence in human affairs; its respect for such qualities as skepticism, tolerance, fair-mindedness, and what George

²¹ Richard M. Andrews, review of Fernand Braudel, *The Mediterranean and the Mediterranean World in the Age of Philip II*, tr. Sian Reynolds (New York, 1972–73), in the *New York Times Book Review*, May 18, 1975.

²² Heinz Hartmann, *Psychoanalysis and Moral Values* (New York, 1960), 23. A curious insight into American historians' current attitudes toward proselyting in the classroom is provided by the recent report of the AHA's Committee on the Rights of Historians, mimeographed (Princeton, 1974). The committee found, on the basis of eight thousand questionnaires circulated in 1971, that members of our profession are sharply divided about their ethical obligations toward students. "Only 49.5% of the respondents thought it impermissible to introduce extraneous material for purposes of indoctrination" (p. 10). This issue is not quite the same as that of expressing value judgments, which surely need not be "extraneous." But the report would seem to indicate that a good bit of open as well as hidden preaching has been going on. The data permit no correlations between the responses and the various value systems of the respondents.

Orwell called (for want of a more precise term) “decency”—these traits combine to make up a world view that in some ways overlaps those of the radical or the conservative, but that possesses its own integrity, its central core of values by which to judge the past and to relate that past to the present.

TO ARGUE THIS CASE is of course to be immediately suspect. Does it not imply a return to what someone has called “nineteenth-century pieties and platitudes,” to the indoctrination of students through the use of selective evidence, and to a downgrading of the search for truth and understanding as the historian’s highest goal? The answer, I think, is that it surely can lead to any or all of these abuses, if misused; but I see no reason to consider such misuse unavoidable. There are dangers built into all stances toward the teaching and writing of history, including the stance called perfect neutrality. Indeed, the liberal historian who applies his values honestly and consistently will be more likely than any other to take pains to consider all the evidence and all alternative interpretations before advancing and defending his own view of the past. What too many of us have hesitated to do, I believe, is to take that final step—to risk a conclusion, to make a judgment, to advance and defend our view of how things were, and why, and what this meant to people of the time, and what it means to people of today.

Clearly there have been some exceptions: some liberals who have not hesitated to assert their values and to let those values suffuse their work as well as their personal conduct. Most of us could probably name a few from among our friends and acquaintances. To offer you my own list would be in egregiously bad taste, but perhaps I may risk one example—partly because of the subject matter involved, but more because of the spirit of the work. A recent volume of essays entitled *The Failure of Illiberalism* deals with society and the state in Imperial Germany; it argues persuasively that the fundamental liberal values were badly stunted in the Germany of that era, and that the result was unfortunate all round. The analysis is not neutral or colorless; it clearly reflects, as one reviewer put it, the author’s own system of values—his belief in “the institutionalization of decency, political playfulness, persuasion, debate, and tolerant dissent.”²³ Subject and spirit are harmoniously woven together; the author’s value base, vigorously held and clearly implicit throughout, inspired the writing of the book and gives the work its originality and force. Such an example provides evidence that consciously liberal history can be written without preaching or distortion, and that it can give us a deepened understanding of a sensitive and controversial subject.

Liberal historians, I believe, also have some obligation to counteract that “competent scoundrel versus honest boob” syndrome that they have unwittingly helped to implant in the public mind. A highly respected teacher of mine (himself a liberal) used to enjoy remarking that “in politics, a man must know how to rise above principle”—something that he himself, in fact,

²³ Peter Loewenberg, review of Fritz Stern, *The Failure of Illiberalism* (New York, 1972), in *AHR*, 78 (1973): 119.

would never have thought of doing. Perhaps so. Still, all but the most hard-core cynics among us can surely come up with examples of public figures who steadfastly refused the temptation to “rise above principle.” True, the liberal historian is not likely to portray any historical figure as a spotless saint, for he is normally inclined to see men and women as paradoxical mixtures—creatures driven by contradictory impulses, not specimens of pure gold or dross.²⁴ Yet even if such complexity is the human condition, it still leaves room for the occasional Abraham Lincoln, or Tomáš Masaryk, or Jean Jaurès. Abandoned to the cynic or the ideologue, the Lincolns and Masaryks and Jaurèses can be—and have been—adroitly transmuted into neurotic hypocrites, self-serving bourgeois, or “honest boobs”; but when considered with sympathy in the light of liberal values they emerge as men whose predominant traits would be regarded as virtues in any age, men who both spoke and acted in defense of the highest of human aspirations. What student of history will not respond to a figure like Jaurès, if offered the opportunity to see him as his admirers did, and as some of us still do? Untouched by vanity, arrogance, or a thirst for power, deeply committed to the Orwellian principle of “decency” (he preferred to call it “integrity”), Jaurès dedicated his enormous energies and talents to what he saw as the cause of human justice. Weighted down by a backbreaking load of obligations as parliamentary leader, newspaper editor, traveling salesman for social democracy, crusader for peace, and spare-time historian, Jaurès in those hectic prewar years could somehow still find time to accept a university’s invitation to lecture on the life and work of Leo Tolstoy. The words of the lecture were his own, not those of a professional speech writer; and they culminated thus:

In our narrow, confined existence, we tend to forget the essence of life. . . . All of us, whatever our occupation or class, are equally guilty: the employer is lost in the running of his business; the workers, sunk in the abyss of their misery, raise their heads only to cry in protest; we, the politicians, are lost in daily battles and corridor intrigues. All of us forget that before everything else, we are men, ephemeral beings lost in the immense universe, so full of terrors. We are inclined to neglect the search for the real meaning of life, to ignore the real goals—serenity of the spirit and sublimity of the heart. . . . To reach them—that is the revolution.²⁵

Do such impassioned words, does such a dedicated life suggest that we are dealing with just another “honest boob”? Some would say so, and their version continues to be heard.²⁶ But that version is hardly the inescapable terminus of the historian’s search for objective truth. Figures like Jaurès are too rare and too important to be left to the cynics or even to the hard-core “realists”; they need to be viewed in the light of liberal values as well, if their real historical significance is to be fully understood.

²⁴ Liberal historians may not agree with all of Alexandr Solzhenitsyn’s public statements, but they are likely to respond to this passage: “If only it were all so simple! If only there were evil people somewhere insidiously committing evil deeds, and it were necessary only to separate them from the rest of us and destroy them. But the line dividing good and evil cuts through the heart of every human being. And who is willing to destroy a piece of his own heart?” *The Gulag Archipelago*, tr. Thomas P. Whitney (New York, 1973), 168.

²⁵ Quoted in Harvey Goldberg, *The Life of Jean Jaurès* (Madison, 1962), 415–16.

²⁶ For example, Jaurès emerges from Guy Chapman’s treatment in *The Dreyfus Case* (London, 1955) as a

DAVID HACKETT FISCHER, in his provocative catalog of historians' sins, has warned us against what he calls the "moralistic fallacy," which, he says, would make history once again the handmaid of moral philosophy—a goal that appeals primarily, says Professor Fischer, to "hairy graduate students."²⁷ Herbert Butterfield has strongly conditioned us against what he called "pseudomoral judgments, masquerading as moral ones, mixed and muddy affairs, part prejudice, part political animosity, with a dash of ethical flavoring wildly tossed into the concoction."²⁸ And John Clive reminds us that Macaulay (of all people) once wrote, "We know no spectacle so ridiculous as the British public in one of its periodical fits of morality."²⁹ Many of my professional colleagues may find that I have been seized by one of those fits, and that I am proposing to sell our birthright for a mess of moralistic pottage. I confess that it is hard to escape a sense of awkwardness after a lifetime of trying to conform to the standards of scientific detachment. Still, as Hugh Stretton reminds us, "Moral abstinence [is] a moral act like any other"; and John Higham adds the pertinent conclusion that what we ought to be after is "thoroughgoing moral criticism" rather than "impressionistic moral judgments."³⁰ It may be the times we live in, or it may be incipient senility, that prods one against his better judgment into reflecting on some of the essentials, and into wondering, as Carl Becker used to do, "what is the good of history?" A valued French colleague remarked to me not long ago that such a question is "terribly American" and asserted that so long as there is a market for what we do, the question is irrelevant. Besides, he added, the question reflects the fact that most Americans are much too moralistic; what they need, for their own and the world's repose, is a large dash of cynicism. He may be right. Yet somehow I remain unregenerate. Perhaps it is a buried aspect of that old liberal heritage, so much maligned in our day; or perhaps it is a surviving spark of an evangelical upbringing. It has not yet driven me to the point of urging that we resurrect the label "moral science" as a category within which our profession might find its proper place. But it does impel me to think that for some of us at least, our search for truth ought to be quite consciously suffused by a commitment to some deeply held humane values. The effort to keep these two goals in balance may be precarious; but if we can manage it, perhaps we will be on the way to re-establishing the role of history as one—and not the least—of what we might fairly call the moral arts.

somewhat ridiculous windbag: "Like many Socialist leaders, he was a bourgeois, and a comfortably situated bourgeois. The things he valued had little interest for the mass of the workers. Like other bourgeois Socialists he wanted to transform them into simulacra of himself, concerned for what he would call the higher values. . . . There are no reforms to which the name of Jaurès is attached. Nothing save eloquence" (p. 330). Nothing save eloquence, indeed! One does not have to descend into hagiography to portray the powerful contemporary impact not only of Jaurès's oratory but also of his personality, his human qualities, and the enduring hold of the Jaurès legend since his day.

²⁷ Fischer, *Historians' Fallacies*, 78–79.

²⁸ Butterfield, *History and Human Relations*, 114.

²⁹ Quoted in John Clive, *Macaulay: The Shaping of the Historian* (New York, 1973), 493.

³⁰ Stretton, *The Political Sciences*, 31–32; Higham, "Beyond Consensus," 622.

Coming to Terms with Samuel Adams

PAULINE MAIER

WHEN JOHN ADAMS REFLECTED on how future historians would remember his cousin Samuel, he was filled with forebodings. Samuel Adams's character "will never be accurately known to posterity," he wrote, "as it was never sufficiently known to its own age." And on October 3, 1803, the day after Samuel Adams's death, a Salem clergyman confided very similar observations to his diary. Adams seemed to have "an impenetrable secrecy," the Reverend William Bentley claimed; he was "feared by his enemies" yet remained "too secret to be loved by his friends."¹

The accuracy of these statements is open to question. They are nonetheless arresting because both observers knew Samuel Adams in life, yet found in him an elusiveness that has evaded his biographers. As the nation moved further from Samuel Adams's lifetime, portraits of him became increasingly confident, even stereotypic, and hostile. There could be no better occasion than this Bicentennial year to re-examine the standard interpretation of Adams, to see how it evolved, to evaluate it against historical evidence, and to reflect on whatever dissonance emerges. The results are important because they open the way for a far different understanding of Adams and, beyond that, of the curious way Americans have recalled their revolutionary past.

ALL STUDIES OF SAMUEL ADAMS turn about one central observation—that his career climaxed in 1776. The son of a Boston maltster, Samuel followed his father into the politics of his town and province. He became a member and soon clerk of the Massachusetts assembly, a leader of the Boston town meeting, and an important person in the informal or extralegal local political groups of the late colonial period. A friend, if not a member, of the Loyal Nine, a club that became Boston's Sons of Liberty in the Stamp Act crisis, he

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¹John Adams to Thomas Jefferson, Quincy, May 27, 1819, in *The Adams-Jefferson Letters*, ed. Lester J. Cappon, vol. 2: 1812–1826 (Chapel Hill, 1959), 541; Bentley, diary, Oct. 3, 1803, in *The Diary of William Bentley, D.D.* (Salem, 1911), 3: 49.

was a leading supporter of the nonimportation effort that opposed the Townshend duties, a spokesman for the town in its effort to expel royal troops after the “massacre,” the organizer of Boston’s Committee of Correspondence, and an important participant in the province’s transition from regular to revolutionary government. He served as a delegate to the Continental Congress and signed the Declaration of Independence.

During these years, both in guiding Massachusetts through the decade before independence and in forging a durable intercolonial union, his importance was of the first rank. Thomas Jefferson called him “truly the *Man of the Revolution*”; John Adams said he was “born and tempered a wedge of steel to split the knot of *lignum vitae*” that tied North America to Great Britain. His exclusion, along with that of John Hancock, from General Thomas Gage’s proclamation of amnesty in 1774 established and heightened his fame, perhaps, as Mercy Warren suggested, beyond what was justified by his actual abilities and contributions. “Everybody in Europe knows he was one of the prime movers of the late Revolution,” the marquis de Chastellux noted in 1780. Little wonder, then, that upon his arrival in France John Adams had found himself constantly confounded with “le Fameux Adams”—cousin Samuel.²

Considering his age when independence was declared, Samuel Adams might well have played a less critical role in the Revolution thereafter. He was fifty-four in 1776, that is, ten years the senior of George Washington, thirteen of John Adams; he was twenty-one years older than Thomas Jefferson, twenty-nine than James Madison, thirty-three than Alexander Hamilton. Yet he served tirelessly on committees of the Continental Congress from its outset until 1781, a period in which the administrative as well as legislative burden of the new nation was borne by a handful of harried delegates. He then returned to Massachusetts, never to leave again. He was chosen president of the Massachusetts senate, lieutenant governor, then governor, an office to which he first succeeded on the death of John Hancock in 1793 and then was himself elected in 1794, 1795, and 1796. These latter offices were, however, granted him partly in recognition of earlier services, which were acknowledged by his current political opponents. Even those who refused to vote for Adams as governor, John Eliot testified in 1809, thought “he did worthily in those times, when instead of building up a government suited to the condition of a people, we had only to pull down a government becoming every day more tyrannical.” Yet “from his age, habits, and local prejudice,” Samuel Adams seemed to many unsuited “to mingle with politicians of a later period, whose views must necessarily be more comprehensive, and whose object was to restrain rather than give a loose to popular feelings.”³

² Jefferson quoted in John C. Miller, *Sam Adams: Pioneer in Propaganda* (Boston, 1936), 343; John Adams to William Tudor, Quincy, June 5, 1817, and Adams, diary, Feb. 11, 1779, in *The Works of John Adams*, ed. Charles F. Adams (Boston, 1850–56), 10: 263; 3: 189–90; Mercy Warren, *History of the Rise, Progress and Termination of the American Revolution* (Boston, 1805), 1: 211–12; François-Jean, marquis de Chastellux, *Travels in North America in the Years 1780, 1781 and 1782*, ed. Howard C. Rice, Jr. (Chapel Hill, 1963), 1: 142.

³ John Eliot, *A Biographical Dictionary* (Salem, 1807), 7.

Samuel Adams's image as a troublemaker, which later generations would develop—a master at pulling down government, at loosing the passions of the people—was already there when Eliot wrote. But another theme was equally, and perhaps more, persistent in the early nineteenth century: Adams was “austere . . . rigid . . . opinionated.” “His conversation was in praise of old times, his manners were austere, his remarks never favourable to the rising generation.” He belonged to another era, continuing to wear the tricorne hat of Revolutionary days, convinced that “the Puritans of New England were the men to set an example to the world.” Eliot's observations were seconded by William Tudor, who was born in 1779, fifty-seven years after Adams, and found that Revolutionary leader a man of “too much sternness and pious bigotry.” He was “a strict calvinist,” Tudor wrote, “and probably, no individual of his day had so much the feelings of the ancient puritans.”⁴ And so Samuel Adams was excluded from the pantheon of Revolutionary leaders around which Americans were asked to rally in the early nineteenth century. His first full biography, by William V. Wells, a descendant, was published only in 1865, more than a half century after John Marshall's *Life of George Washington* went to press and Mason Weems began producing his popular panegyrics to Washington and other Revolutionary heroes.⁵

John Adams noted this neglect with disapproval. “If the American Revolution was a blessing, and not a curse,” he wrote in 1819, “the name and character of Samuel Adams ought to be preserved. It will bear a strict and critical examination even by the inveterate malice of his enemies. A systematic course has been pursued for thirty years to run him down.” Constantly John defended Samuel, in conversations with the Englishman Richard Oswald against extravagant versions of “trouble-making”—“You may have been taught to believe . . . that he eats little children; but I assure you he is a man of humanity and candor”—and later against the more personal charges of William Tudor. If Samuel was stern, “a man in his situation and circumstances must possess a large fund of sternness of stuff, or he will soon be annihilated.” As for bigotry, “he certainly had not more than Governor Hutchinson and Secretary Oliver,” his old opponents, but “lived and conversed freely with all sectarians,” never seeking to proselytize. Samuel was of course a Calvinist; “a Calvinist he had been educated, and so had been all his ancestors for two hundred years.”⁶ Already, it seemed, interpretations of Samuel Adams were being distorted because a younger generation had lost touch with a world so soon gone and imposed upon the dead its own, more modern expectations. Yet John himself was responding to an issue that had only recently taken on importance: was, or was not, Samuel Adams a suitable

⁴ *Ibid.*, 6, 16; Stewart Beach, *Samuel Adams: The Fateful Years, 1764–1776* (New York, 1965), 310; John Adams to William Tudor, Quincy, June 5, 1817, in *Works of John Adams*, 10: 262; William Tudor, *The Life of James Otis* (Boston, 1823), 274–75.

⁵ William V. Wells, *The Life and Public Services of Samuel Adams* (Boston, 1865); John Marshall, *The Life of George Washington* (Philadelphia, 1804–07); Mason Weems, *The Life of Washington*, ed. Marcus Cunliffe (1808; rpt., Cambridge, Mass., 1962).

⁶ John Adams quoted in Wells, *Samuel Adams*, 1: v; John Adams, diary, Nov. 15, 1782, and Adams to Tudor, Quincy, June 5, 1817, in *Works of John Adams*, 3: 310; 10: 262.

national hero? To a large extent that problem remains central in writings about him, and so historians have had to consider not only the human reality of their subject, but also his appropriateness as a model for modern Americans.

When Wells finally wrote Samuel Adams's biography, he tried to undo the damage of neglect, to restore Adams's name as "a necessity to those who revere virtue and exalted patriotism." For Wells's generation, "the righteous principle of the Revolution" was assumed. It remained only to stress Adams's role. And so Wells accepted uncritically George Bancroft's assertion that Samuel was an early advocate of independence. Adams "knew no political creed but absolute, unconditional independence," Wells claimed. "He hungered and thirsted after it' as an object of priceless attainment, in comparison to which all else on earth was of secondary importance," and in this he was distinguished from his colleagues in the revolutionary movement who sought to avert separation on into the 1770s. For Wells this was the stuff of heroism, and so he readily conceded that Samuel Adams was the "Arch Manager" of the revolutionary movement, the "Chief Incendiary." But did "all contemporary evidence" show that Adams deserved those titles, as Wells suggested? Royal officials and loyalist writers had long ascribed the revolutionary movement to a faction of disaffected colonists, but Samuel Adams was only one of several Americans they cited for seditious activities. Adams's later reputation owed much to Gage's proclamation, to the explicit accusations of a few loyalists, and to his Federalist opponents of the 1790s who found his sympathy for the French Revolution and Jeffersonianism in keeping with an earlier identity as "grand mob-leader during the Revolution." Wells shared the Federalists' distaste for contemporary upheaval: despite his toleration of colonial insurgency, he was anxious to dissociate Samuel Adams from the Southern Confederate "revolutionaries" of the mid-nineteenth century, which he accomplished by stressing Adams's understanding of federalism.⁷

James I. Hosmer's *Samuel Adams* (1885) remained in the nineteenth-century tradition of filiopietistic biographies: the author noted that his great-great-grandfather had served with Samuel Adams in the revolutionary struggle. Adams remained a hero of sorts, but Hosmer was disturbed by the means

⁷ Wells, *Samuel Adams*, 2: 302-03; 1: vi; 3: 274-75, 351n. Cf. George Bancroft, *A History of the United States* (Boston, 1834-74), vol. 6 (10th ed.; Boston, 1859). For loyalist accusations of Samuel Adams, see particularly Peter Oliver's *Origin & Progress of the American Rebellion: A Tory View*, ed. Douglass Adair and John Schutz (Stanford, 1961), 39-41, and Joseph Galloway, *Historical and Political Reflections on the Rise and Progress of the American Rebellion* (London, 1780), 67-68. Professor Mary Beth Norton of Cornell University has suggested to me that loyalist writers cited Benjamin Franklin more often than Samuel Adams as arch conspirator of the American "rebellion." When in 1766 Thomas Hutchinson wrote of "our grand incendiary," he referred to James Otis, Jr., not Samuel Adams. So late as 1773 Adams appears in Hutchinson's correspondence only as a particularly influential member of a political faction allegedly disloyal to Britain. See Hutchinson to Richard Jackson, Boston, Nov. 16, 1766, and to Dartmouth, Oct. 9, 1773, Massachusetts Archives, State House, Boston, 26: 253; 27: 549-50. Historians also cite Hutchinson's statement that in 1765 Adams "owned, without reserve, in private discourse" that he sought independence "and from time to time made advances towards it in public, so far as would serve the great purpose of attaining it." In context, however, it is clear Hutchinson meant that Adams sought an independence of Parliament's sovereignty, not of Britain more generally. See Hutchinson's *History of the Colony and Province of Massachusetts Bay*, ed. Lawrence S. Mayo (Cambridge, Mass., 1936), 3: 96-97.

Adams used to convert his countrymen to independence, which, Hosmer wrote, he had “begun to cherish” in the 1760s. James Rivington’s charge that Adams had a “Machiavellian streak in his character” seemed too strong, but like all New Englanders, Hosmer said, Adams “stooped now and then to a piece of sharp practice.” This was “never for himself, but always for what he believed the public good.” Still, it was a defect. The publication in 1773 of private letters by Governor Thomas Hutchinson, Lieutenant Governor Andrew Oliver, Charles Paxton, and others was particularly cited as “the least defensible proceeding in which the patriots of New England were concerned during the Revolutionary struggle.” “Nothing,” Hosmer claimed, “can be more sly than the manoeuvring throughout.” He in fact found it much easier to admire Hutchinson, whose biography he subsequently wrote.⁸

From Hosmer’s impatience with Yankee trickery, biographers became increasingly hostile toward Adams. His still unquestioned early commitment to independence rapidly lost heroic attributes. For Ralph Volney Harlow, whose *Samuel Adams: Promotor of the American Revolution* was published in 1923, all of Adams’s actions seemed irrational, the effusions of a psyche described as neurotic, even psychotic. Before 1764, Harlow claimed, Adams had failed in all he tried, which produced “a pronounced conviction of inadequacy, or an ‘inferiority complex.’” Then he drafted Boston’s instructions to her legislative representatives and suddenly found a cause in the Anglo-American controversy. His “extraordinary activity after 1765” was explained “as the result of his unconscious efforts to satisfy his hunger for compensation, and to bring about a better adjustment to his environment.” By implication, independence was attributed entirely to Adams’s derangement—no real problems lay behind that event. “It was something inside, rather than outside which drove him on, something in the field of the unconscious.” Adams turned to politics only to find “relief from his tiresome mental problems.” Often, Harlow suggested, followers see their leader “as a heroic patriot, when he may be only a neurotic crank,” one who, in this case, found it “easy . . . to manufacture public opinion with a pen.”⁹

The notion that Samuel Adams somehow “manufactured” the Revolution by manipulating people appeared again four years later in Vernon Parrington’s *Colonial Mind*. Parrington found Adams a “professional agitator,” “an intriguing rebel against every ambition of the regnant order,”

⁸ James I. Hosmer, *Samuel Adams* (Boston, 1885; rpt., 1896), viii–ix, 68, 368–69, 229. See also, Hosmer, *The Life of Thomas Hutchinson* (Boston, 1896). For a full account of the affair of the Hutchinson letters, see Bernard Bailyn, *The Ordeal of Thomas Hutchinson* (Cambridge, Mass., 1974), 223–57. In short, the letters were received from Benjamin Franklin with the stipulation that they not be published. On June 2, 1773, Adams read them to the assembly, which condemned them as an effort to overthrow the colony’s constitution and introduce arbitrary rule. Exaggerated rumors of their contents circulated until even Andrew Oliver believed they should be published. Then on June 10 Adams reported to the legislature that a separate set of letters had appeared in Boston—clearly a ruse to bypass Franklin’s restriction and to permit the publication of the letters, which was finally ordered on June 15. The published letters distorted Hutchinson’s position, Bailyn suggests, because those letters that reached Boston had been selected in *England* several years earlier to buttress a political argument at odds with Hutchinson’s views.

⁹ Ralph Volney Harlow, *Samuel Adams: Promotor of the American Revolution* (New York, 1923), 36–38, 64, 65, 37.

but could condone those roles since he believed Adams sought not only independence but, beyond that, a democratic republic. Doubts that ends could justify means soon reappeared, however. Manipulation was a central theme of John C. Miller's *Sam Adams: Pioneer in Propaganda* (1936), which remains the most scholarly of modern biographies. Miller wrote of Adams "transforming American discontent into revolutionary fervor." He was the puppeteer who "brought the people to approve his schemes and pulled the wires that set the Boston town meeting in motion against royal government," who created the convention of towns in 1768 "as a steppingstone to a later usurpation of governmental power," and then "deliberately set out to provoke crises that would lead to the separation of mother country and colonies." Finally, "by transplanting the caucus from Faneuil Hall to Philadelphia," Adams, working behind the scenes, "directed every step toward independence." This interpretation was reduced to stereotype in the portrait of Samuel Adams prepared for *Sibley's Harvard Graduates* (1958) by Clifford Shipton, who accepted uncritically the accounts by Harlow and Miller and produced forty-five pages of contempt. Adams, Shipton wrote, "preached hate to a degree without rival" among New Englanders of his generation: "He taught his dog Queue to bite every Red Coat he saw, and took little children to the Commons to teach them to hate British soldiers."¹⁰

The tide may have begun to turn. In an article of 1960, William Appleman Williams quickly dismissed earlier explanations of Adams's politics, then sketched out the elements of a new and far more sympathetic interpretation. Williams argued, in short, that Adams "became a revolutionary . . . because he was a Calvinist dedicated to the ideal and the reality of a Christian corporate commonwealth." Five years later Stewart Beach's *Samuel Adams: The Fateful Years* broke with the major assumptions of Adams's previous biographers. Beach questioned whether Adams sought independence before the mid-1770s. (Williams, by contrast, said Adams "clearly sought independence after 1769.") He acknowledged that Adams could not alone control Boston's Sons of Liberty and rejected outright the common notion that Adams was "a rabble-rousing demagogue who stood on street corners in Boston directing the mob." Beach, moreover, tried to depart from the entire framework of interpretation that was established in the early nineteenth century. "It is not necessary," he said, "to approach Samuel Adams as a hero to find him an intensely human and fascinating individual." Although on

¹⁰ Vernon Parrington, "Samuel Adams, The Mind of the American Democrat," in *The Colonial Mind, 1620-1800* (New York, 1927), 233-47, especially 233; Miller, *Sam Adams*, 144, 152, 276, 342; Clifford Shipton, "Samuel Adams," in *Sibley's Harvard Graduates* (Boston, 1958), 10: 420-65, especially 463, 434. Shipton probably founded one of his accusations upon an incident involving John Quincy Adams. See John to Samuel Adams, Auteuil, France, Apr. 27, 1785: "The child whom you used to lead out into the Commons to see with detestation the British troops, and with pleasure the Boston militia, will have the honor to deliver you this letter. He has since seen the troops of most nations in Europe, without any ambition, I hope, of becoming a military man. He thinks of the bar and peace and civil life." Samuel considered the episode a lesson in patriotism, not hate. He replied from Boston, April 13, 1786, that "the child whom I led by the hand, with a particular design, I find is now becoming a promising youth. . . . If I was instrumental at that time of enkindling the sparks of patriotism in his tender heart, it will add to my consolation in the latest hour." Letters quoted in Wells, *Samuel Adams*, 3: 220.

many issues Beach's arguments were founded upon a closer sifting of evidence than were those of his predecessors, the biography he produced, like Williams's essay, did not cite its sources and has had no scholarly impact.¹¹

By contrast, Richard D. Brown's study of the Boston Committee of Correspondence (1970) is a book to be contended with by any who continue to hold what Charles W. Akers recently called "the myth of Sam Adams as the Boston dictator who almost singlehandedly led his colony into rebellion." Boston politics, Brown demonstrated, were a "mixture of planning and spontaneity." Similarly, the capital's relationship with outlying towns was too reciprocal, the restrictions on central leadership were too pervasive to justify any simple interpretation of politics founded upon Adams's control. Clearly events look different when historians, as Akers urged, "attribute to Samuel Adams only those actions, influences, intentions, and writings for which there is reasonably direct and certain evidence."¹² Yet the older view persists—in books like Hiller Zobel's *Boston Massacre* (1970), which rejected in the person of Samuel Adams all who were concerned with "percolating public dissatisfaction with the established order"; in numerous books for children inspired by the Revolutionary Bicentennial; in the catalog for a special exhibition in Adams's home town on "Paul Revere's Boston," which identifies Adams as "a central figure in stirring up mob violence";¹³ in popular consciousness. Historians have, then, re-examined and questioned the so-called Adams myth, but have not yet abandoned or overturned it.

SAMUEL ADAMS WAS THE FIRST to seek American independence. He was a propagandist who manufactured the Revolution by techniques of mass manipulation. He was responsible for mobs and popular violence. These three propositions about Adams's political career have evolved slowly and powerfully to determine public views of him as a historic person. They have a particular fascination because his own writings and actions suggest more complex and, in some cases, directly contrary conclusions.

No revolutionary discovered independence. John Adams particularly laughed at the affectation of representing it as "a novel idea, . . . a late invention," since, he claimed, the idea of separation "sooner or later" was "always familiar to gentlemen of reflection." A recent study by J. M. Bumstead confirms that independence was discussed by both English and American writers long before the 1770s. The issue, then, is when colonists

¹¹ William Appleman Williams, "Samuel Adams: Calvinist, Mercantilist, Revolutionary," *Studies on the Left*, 1 (1960): 50, 52; Beach, *Samuel Adams*, 9, 78, viii, x.

¹² Richard D. Brown, *Revolutionary Politics in Massachusetts: The Boston Committee of Correspondence and the Towns, 1772-1774* (Cambridge, Mass., 1970), 124n., *passim*; Charles W. Akers, "Sam Adams—And Much More," *New England Quarterly*, 47 (1974): 120-31, especially 120, 130. Akers demonstrates the persistence of the Adams myth in works that are, for the most part, other than the biographies considered here.

¹³ Hiller B. Zobel, *The Boston Massacre* (Boston, 1970), 57. On children's books, see, for example, Don Lawson, *The American Revolution* (New York, 1974); Milton Lomask, *The First American Revolution* (New York, 1974); and Isaac Asimov, *The Birth of the United States, 1763-1816* (Boston, 1974). See also *Paul Revere's Boston, 1735-1818* (Boston, 1975), 124, pl. 172.

decided upon independence as an immediate goal and began to work toward it. Biographers agree that Samuel Adams took those steps earlier than others but disagree upon just when he did so. Wells, like Bancroft, ascribed the event to 1768, when British troops arrived in Boston. Miller also accepted that date; Hosmer suggested Adams's conversion occurred somewhere between 1765 and 1768, while Harlow inclined toward 1765, and Shipton simply observed that "from the beginning of his political career he was accused of being for independence, and later he boasted that this was so."¹⁴ Adams's own writings suggest that his thinking on independence evolved more gradually and can best be described as occurring in three stages with relatively distinct chronological barriers: he moved from disavowal, to prediction or warning, to advocacy of American independence.

Nothing in Adams's writings before, during, or immediately after the Stamp Act crisis (1765–66) suggests a desire for independence. His earliest known political writings—from the 1740s—include fulsome praise of the British constitution. He admitted, however, a significant "prejudice" in favor of Massachusetts government, which was modeled on that of England, but with improvements; because New England's founders "had so severely felt the effects of tyranny," he wrote, they secured for their descendants not only all the standard English liberties but "some additional privileges which the common people there have not."¹⁵ The colonists' demand that they be taxed only by their own representatives, even their resistance to the Stamp Act, seemed to him in perfect accord with British tradition. There was no reason to doubt that colonists would continue "faithfull & loyal Subjects," he wrote in 1765—were they allowed the same governmental powers to which they had long been accustomed, powers he understood to be those not of a sovereign state but a "subordinate civil Governmt." Adams's disavowal of independence reached the height of explicitness in a letter he drafted for the Massachusetts assembly to Lord Rockingham, dated January 22, 1763: the House and its constituents were "so sensible . . . of their happiness and safety, in their union with, and dependence upon, the mother country, that they would by no means be inclined to accept an independency, if offered to them."¹⁶

Thereafter the situation rapidly became more serious. The arrival of British troops at Boston in the fall of 1768 was of particular importance; Adams was always a bitter foe of standing armies, whose use against civilians in time of peace he, like other Englishmen, considered a major sign of impending tyranny. Other measures also hastened his reassessment of the colonies' position. In promising to pay Crown appointees with customs revenues, the Townshend

¹⁴ John Adams to Benjamin Rush, Quincy, May 21 and 1, 1807, in *Works of John Adams*, 9: 596, 591; J. M. Bumstead, " 'Things in the Womb of Time': Ideas of American Independence, 1633 to 1763," *William and Mary Quarterly*, 3d ser., 31 (1974): 533–64; Wells, *Samuel Adams*, 1: 207; George Bancroft, *History of the United States*, 6: 192; Miller, *Sam Adams*, 229; Hosmer, *Samuel Adams*, 68, 120; Harlow, *Samuel Adams*, 87–88; Shipton, "Samuel Adams," 427.

¹⁵ Adams, essay from the *Independent Advertiser*, in Wells, *Samuel Adams*, 1: 21–22.

¹⁶ Adams to Dennis DeBerdt, Boston, Dec. 16, 1766; to G— W—, Boston, Nov. 13, 1765; and assembly letter, in *The Writings of Samuel Adams*, ed. Harry A. Cushing (Boston, 1904–08), 1: 113, 39, 170.

Act (1767) threatened both the colonial assemblies' exclusive right to tax their constituents and their traditional role as paymaster, by which the legislatures had exercised a crucial check on executive power. It was rumored, then confirmed, that the Crown would pay the Massachusetts governor and judges. Meanwhile the removal of the General Court from Boston to Cambridge by the acting governor, Thomas Hutchinson, and his surrender of the harbor garrison at Castle William to the Royal Army suggested that the colony's executive officer was no longer so independent an agent as his predecessors had been, but now acted on orders from London, even when they conflicted with the Massachusetts Charter.¹⁷ The danger of colonial government by "ministerial mandates" was equal to that of parliamentary taxes or standing armies, Samuel Adams warned; the dissolution of popular checks on the governor made him a tyrant, and the addition of judges to the Crown payroll completed the transformation of the free government of Massachusetts into a despotism. Nor was the problem confined to the Bay Colony: the Gaspée Commission's infringement of jury rights in Rhode Island (1772) showed that the menace of executive power surmounted provincial boundaries, while events abroad, particularly in Ireland and England—which Adams followed closely—seemed to prove that the threat of despotism permeated the empire. The effort to undermine democratic checks on executive power was not, Adams thought, new. It went back perhaps to the British administration of Sir Robert Walpole. But the spate of recent advances made the danger urgent. The entire British world seemed on a precipice; tyranny was at the door.¹⁸

Under the force of these unfolding events, Adams moved toward predicting independence, warning that it was an increasingly possible outcome of the Anglo-American conflict. In an article signed "Alfred," published in the *Boston Gazette* on October 2, 1769, for example, he expressed fears that the "Jealousy between the mother country and the colonies" first raised in the Stamp Act crisis might "finally end in the ruin of the most glorious Empire the sun ever shone upon." But hopes for a changed British policy became ever dimmer. By October 1771 Adams wrote his trusted friend Arthur Lee, then in London, "I have no great Expectations from thence, & have long been of Opinion that America herself under God must finally work out her own Salvation." Independence might, however, be far off. To the Rhode Island radical Henry Marchant, Adams wrote of it in 1772 only as a probability for "some hereafter." He saw no reason either side should hasten the crisis. "I am a friend to both," he wrote, "but I confess my friendship to [the colonies] is the most ardent."¹⁹

¹⁷ For an account of these events, see Bailyn, *Ordeal of Thomas Hutchinson*, 169–75.

¹⁸ Adams as "A Chatterer," in *Boston Gazette*, Dec. 3, 1770; Adams to Darius Sessions, Boston, Dec. 28, 1772; to Arthur Lee, Boston, Sept. 27, 1771; and as "Candidus," in *Boston Gazette*, Oct. 14, 1771, in *Writings of Samuel Adams*, 2: 70–71, 389–92, 231–32, 252. On the importance of events abroad in American disillusionment with Britain, see Pauline Maier, *From Resistance to Revolution: Colonial Radicals and the Development of American Opposition to Britain, 1765–1776* (New York, 1972).

¹⁹ Adams as "Alfred," in *Boston Gazette*, Oct. 2, 1769; to Lee, Oct. 31, 1771; to Henry Marchant, Jan. 7, 1772, in *Writings of Samuel Adams*, 1: 387; 2: 267, 309.

Prediction, therefore, fell short of advocacy. Always Adams's forecasts of independence were contingent. As he said in a letter to Arthur Lee of April 1774, separation would come only "if the British administration and government do not return to the principles of moderation and equity." During the early 1770s this seemed possible as the colonists' confidence in the mother country was not yet "in too great a Degree lost." Adams's disillusionment did not yet extend beyond the Parliament and ministry to the king or nation at large. And so his position was reformist, for "a Change of Ministers & Measures," not for so revolutionary a transformation as independence implied.²⁰ He worked actively, moreover, for reform. Even his predictions were phrased as warnings to Britain, designed to awaken the mother country to the serious consequences her actions portended, and so to inspire political change. The publication in 1773 of private letters by Hutchinson and other royal officials, which so disturbed Hosmer, was itself an effort to facilitate reconciliation. Adams, like others of his colleagues, believed the current campaign against American freedom had been inspired by "a few men born & educated amongst us, & governd by Avarice & a Lust of power" and later "adopted" by Britain. If these men—now so fully exposed and condemned by their own words—could be removed from office on the demand of outraged Massachusetts freemen, "effectual measures might then be taken to restore 'placidam sub Libertate Quietam,' " a peace consonant with liberty. It might be necessary, however, that some in England also be "impeached & brot to condign punishment."²¹

Adams's interest in cooperation between English and American opponents of Crown policy also argues strongly—more so perhaps than his explicit disavowals of independence—that he wanted reform within the context of empire in the early 1770s. He personally wrote to the English radical John Wilkes in December 1770 and subsequently carried on an active correspondence with the American Wilkesite, Arthur Lee. "The Grievances of Britain & the Colonies . . . are of the same pernicious Growth," he wrote Lee in September 1771, and so the cooperation of patriots in both countries should "by all means . . . be cultivated." His earliest proposal for a correspondence union was designed to facilitate just such a coordination of patriotic activities throughout the empire. Three years later he continued to emphasize the importance of coordinating American and British efforts against the growth of Crown power. In 1774 he also discussed with Lee the terms of a possible American bill of rights, which might have made possible the Americans' continued participation in the British governmental system.²² Within two years, however, the lack of such a document "fixing" Americans' rights under

²⁰ Adams to Lee, Boston, Apr. 4, 1774, and to Marchant, Jan. 7, 1772, in *ibid.*, 3: 100, 2: 309. For other contingent predictions of independence, see *ibid.*, 3: 101, 66.

²¹ Adams to Lee, June 19 and 21, 1773, in *ibid.*, 3: 42, 44. The purpose behind the letters' publication, then, was essentially the same as that of Benjamin Franklin in sending the letters to Massachusetts. Only subsequently did Franklin and Adams lose faith in the empire. See Bailyn, *Ordeal of Thomas Hutchinson*, 233–38.

²² Adams to Lee, Sept. 27, 1771, and Apr. 4, 1774, in *Writings of Samuel Adams*, 2: 234; 3: 101.

the British constitution, and the apparent impossibility of achieving one, had become a major pillar of his case for colonial separation from Britain.

When, then, did Samuel Adams become an advocate of independence? In November 1775, well after the war had begun, he finally wrote James Bowdoin that he could no longer “conceive that there is any room to hope from the virtuous efforts of the people of Britain” against a “tyrant . . . flushed with expectations from his fleets & armies” and possessed of an “unalterable determination, to *compel* the colonists to *absolute* obedience.” America, he wrote James Warren, must send her very best men to Congress, those “fit to be employed in *founding Empires*.” By January 1776 he was acutely distressed by evidence that Portsmouth, New Hampshire, was opposed to separation, and wrote John Adams of his efforts in Congress to prevent further disavowals of independence. The next month he published a newspaper essay forthrightly arguing for separation: the lack of any “Britannico-American Magna Charta” stating precisely the terms of America’s limited dependence meant the colonies faced “an indefinite dependence upon an undetermined power,” currently exercised by “a combination of usurping innovators” who had “established an absolute tyranny in Great Britain and Ireland, and openly declared themselves competent to bind the Colonies in all cases whatsoever.” America was in fact independent; the administration had “dissevered the dangerous tie.”²³

This chronology is not extraordinary. Thomas Jefferson, Elbridge Gerry, and others of their generation went through much the same progression, and their disillusionment with Britain was inspired by many of the same events. Indeed, Adams was less anxious for a declaration of independence than several of his fellows home in Massachusetts. “Let us not be impatient,” he counseled Joseph Hawley. “It requires Time to convince the doubting and inspire the timid.”²⁴ And on July 9, 1776, he wrote Hawley that had independence been declared nine months earlier “we might have been justified in the Sight of God and Man”—then altered the sentence so it read three months instead of nine! Thereafter in letters to his most trusted friends, Adams continued to rethink this question of when independence should have been declared, even though the question was by then academic. He would have been more satisfied, he suggested in December 1776, had the declaration immediately followed Lexington and Concord. But on the whole, he was inclined to think that the course actually taken was best. He had once believed an earlier declaration would have invigorated the American Northern Army and so brought Canada into the Union, “but probably I was mistaken. The Colonies were not then all ripe for so momentous a Change.”²⁵

²³ Adams to James Bowdoin, Philadelphia, Nov. 16, 1775; to James Warren, Philadelphia, Nov. 4, 1775; to John Adams, Philadelphia, Jan. 15, 1776; and article signed “Candidus,” in *Boston Gazette*, Feb. 3, 1776, in *ibid.*, 3: 241, 234–35, 258–59, 261–66.

²⁴ Adams to Joseph Hawley, Apr. 15, 1776, in *ibid.*, 3: 281. See also Adams to Benjamin Kent, Philadelphia, July 27, 1776, in *ibid.*, 3: 305: “Perhaps if our Friends had considered how much was to be previously done they wd not have been, as you tell me some of them were, ‘impatient under our Delay’” in declaring independence. On the colonial leaders’ conversion to independence, see Maier, *From Resistance to Revolution*, 228–70.

²⁵ Adams to Hawley, Philadelphia, July 9, 1776; to Warren, Baltimore, Dec. 21, 1776; to Kent, Philadel-

When weighed against the testimony of Adams's collected writings of nearly three decades, evidence that he was long dedicated to independence is notably weak. The fullest case for that position was made by George Bancroft in the sixth volume of his *History of the United States*, which, through its influence on Wells, has shaped all subsequent discussion of the issue. Bancroft, however, misread Adams's writings, inferring an avowal of independence even from letters that explicitly assert the contrary,²⁶ and he depended heavily—like subsequent biographers—upon the testimony of a Boston innkeeper, Richard Silvester, which was taken under circumstances that severely limit its credibility.²⁷ In short, no evidence that Samuel Adams passionately repudiated Britain before late 1775 compares with an entry in John Adams's diary for December 21, 1772, when John held a heated discussion of the Gaspée Commission with an Englishman. "I said there was no more justice left in Britain than there was in hell," he recalled; "that I wished for war, and that the whole Bourbon family was upon the back of Great Britain; avowed a thorough disaffection to that country; wished that any thing might happen to them, and, as the clergy prayed of our enemies in time of war, that they might be brought to reason or ruin."²⁸ Yet posterity remembers John Adams as conservative, dignified, and safe, in part, no doubt, because he has never been accused of effecting his anti-British feelings by pulling down the standing order. He was, moreover, a Federalist, particularly immune to the taint of "Jacobinism" during years critical in defining how Americans would recall their Revolution.

The biographers' second argument, that Samuel Adams manipulated colonists into independence, depends upon their assumption that he was long dedicated to separation from Britain. Adams could not consciously maneuver the population toward a goal he did not yet espouse. There is no doubt, on the other hand, that Adams sometimes explained the function of public leaders in

phia, July 27, 1776; see also letters from Philadelphia to R. H. Lee, July 15, 1776; to Warren, July 16, 1776; to John Pitts, July 17, 1776; and from Baltimore to Arthur Lee, Jan. 2, 1777, in *Writings of Samuel Adams*, 3: 205, 338, 304-05, 297-98, 299, 300-01, 339-40. The last statement is consonant with a tendency of Adams to consider his own position as mistaken if it was rejected by a democratic legislature.

²⁶ Compare the original text of a letter from Adams to John Wilkes, Boston, Dec. 28, 1770—available in the British Museum Additional Manuscripts 30871 and in *Writings of Samuel Adams*, 2: 100-01—with Bancroft's use of it in his *History of the United States*, 6: 385. (Bancroft also incorrectly dated the letter December 27; see p. 358 n.5.) Bancroft again inferred a dedication to independence from Adams's letter to Arthur Lee of April 4, 1774, although Adams explicitly wished for "a permanent union with the mother country," if that were possible "on the principles of liberty and truth." See *ibid.*, 6: 524, and the text of the original letter in Richard Henry Lee, *Life of Arthur Lee, LL.D.* (Boston, 1859), 2: 215-20.

²⁷ For citations of Silvester, see Bancroft, *History of the United States*, 6: 194, and biographies of Adams by Wells, 1: 209-11; Hosmer, 117-19; Harlow (who distrusted the testimony), 123; Miller, 144-45; and Shipton, 432-33. Silvester swore, in short, that in 1768 he personally heard Samuel Adams avow his desire for independence and a republic. Similar statements were attributed to Dr. Benjamin Church and Thomas Chase, both popular leaders in Boston. The deposition was taken by Thomas Hutchinson in January 1769 during a campaign by Governor Francis Bernard to prove that Boston's disorders were the work of a small disaffected faction; see Bailyn, *Ordeal of Thomas Hutchinson*, 128 n.23. Harlow, although one of Adams's most extreme critics, noted there is no corroboration of Silvester's statement, which "probably . . . simply represents in rather compact form the suspicions and fears of the conservatives" (p. 123). And Beach, on page 17 of his *Samuel Adams*, recalled that not even Hutchinson used the document in his *History of Massachusetts Bay*. This was perhaps because Hutchinson understood that "small dependence . . . can be placed upon ex-parte witnesses, examined by men engaged in political contests." See Hutchinson, *History of Massachusetts Bay*, 3: 199.

²⁸ John Adams, diary, Dec. 21, 1772, in *Works of John Adams*, 2: 308-09.

terms that resemble modern behaviorism: a politician, he wrote in 1778, carefully tries to make men's "Humours and Prejudices, their Passions and Feelings, as well as their Reason and Understandings subservient to his Views of publick Liberty and Happiness." He consistently held that men were ruled more by their feelings than by reason, that the people could in the short run be deluded or mistaken, that when their passions were aroused, the masses were capable of great "tumults," comparable to the ragings of the sea, and they were then as open to reason as "the foaming billows . . . to a lecture of morality and . . . quiet." Still, he saw profound limitations upon the masses' malleability and irrationality. He denied that the people were an "unthinking herd." Calm would succeed disorder; and prudent patriots could help recall the people from prejudice and passion to the exercise of reason. Adams consistently emphasized the people's ability to make valid political judgments, particularly when they acted as a body and had sufficient time for reflection. "The inhabitants of this continent are not to be dup'd," he wrote; "They can judge, as well as their betters, when there is a danger of *slavery*."²⁹

The "true patriot," then, did not—indeed, could not—create disaffection. The task of a popular leader, as Adams explained it, was to explore the causes of popular discontent and then, if he found his country's "*fears and jealousies*" were well grounded, to encourage them "by all proper means in his power." He would "keep the attention of his fellow citizens awake to their grievances; and not suffer them to be at rest, till the causes of their just complaints are removed." Resistance demanded a concerned populace, ready to defend its freedom, and, above all, an abundance of provocation. Indeed, America's enemies seemed far more effective than her friends in hastening colonial union: the Boston Port Act, Adams wrote, like the cannonading of Norfolk, Virginia, "wrought a Union of the Colonies which could not be brot about by the Industry of years in reasoning on the necessity of it for the Common Safety." The Boston Committee of Correspondence, which Adams founded, embodied this attitude. The stated purpose of the committee was to survey public opinion upon British actions, of which the committee took pains to inform its rural correspondents. Since the towns' responses strongly suggested they shared Boston's viewpoint, the committee simply reinforced local patriotism by a sophisticated system of flattery: quotations from a town's previous letter were, for example, often incorporated in Boston's reply along with fulsome statements of approval.³⁰

If Samuel Adams cannot be called the first for independence, if his beliefs and techniques as a popular leader belie the modern notion that he "manufactured" the Revolution by manipulating a mindless people toward

²⁹ Adams to Samuel Cooper, Philadelphia, Dec. 25, 1778, and Apr. 30, 1776; to Elbridge Gerry, Boston, Mar. 25, 1774; as "Candidus," in *Boston Gazette*, Apr. 12, 1773; Boston Committee of Correspondence to Marblehead Committee, Boston, Apr. 12, 1774; as "Vindex," in *Boston Gazette*, Dec. 21, 1771, in *Writings of Samuel Adams*, 4: 107; 3: 284, 83, 29, 96; 2: 148–50.

³⁰ Adams as "Vindex," in *Boston Gazette*, Dec. 21, 1771; to Cooper, Philadelphia, Apr. 30, 1776; and to Warren, Philadelphia, Jan. 7, 1776, in *ibid.*, 2: 148–50; 3: 284, 253–54; Brown, *Revolutionary Politics*, 126–31, 244–45.

an independence without cause, was he at least responsible for popular violence—a man who, as Miller wrote, scored “triumphs” like the Boston Massacre and Tea Party? There were within the revolutionary movement men prone to the use of direct force. But to these Adams preached restraint: patience, he reminded the fiery Thomas Young, marks a patriot. His famous Master of Arts declaration in 1743 affirmed only that it was “Lawful to resist the Supreme Magistrate, if the Commonwealth cannot otherwise be preserved”—not so unacceptable a proposition even in more submissive times. Although Samuel Adams was “staunch and stiff and strict and rigid and inflexible in the cause,” he was always for “softness and delicacy, and prudence,” John Adams testified, “where they will do.” Where they would not, he justified forcible resistance, but only if it fitted defined criteria of acceptability. He was as ready to condemn “a lawless attack upon property in a case where if there had been right there was remedy” as he was to defend “the people’s rising in the necessary defence of their liberties, and deliberately, . . . rationally destroying property, after trying every method to preserve it, and when the men in power had rendered the destruction of that property the only means of securing the property of *all*.” As such he approved the Stamp Act uprising of August 14, 1765, since the cause was important, resistance had widespread support—the “whole People” thought their essential rights invaded by Parliament—and all legal means of redress had been tried to no effect. But he condemned the attack on the homes of Thomas Hutchinson and others on August 26, 1765, as a transaction of “a truly *mobbish* Nature.” There is no evidence that he prompted the Boston Massacre riot, although he served thereafter as spokesman for the town in demanding that troops be removed from Boston. Adams is said to have signaled the Boston Tea Party, and, although his precise role on December 6, 1773, is disputable, the words attributed to him in the final “Tea Meeting” are in perfect accord with his philosophy: “This meeting can do nothing further to save this country.” In effect, all peaceful means of preventing payment of the tea duty, and accepting all it implied, had been exhausted. Only then was the destruction of property justified.³¹

But violence was not his cause. Samuel Adams was above all a master politician, an organizer and coordinator who believed in constitutional government. Already in 1748 he affirmed that “the true object of loyalty is a good legal constitution,” an opinion he sustained through old age. He advised moderation and prudence because these were instruments of political

³¹ Adams to Thomas Young, Philadelphia, Oct. [17], 1774, in *Writings of Samuel Adams*, 3: 163; John Adams, diary, Dec. 23, 1765, in *Works of John Adams*, 2: 163; Samuel Adams to Elbridge Gerry, Mar. 24, 1774, and to John Smith, “20th 1765,” in *Writings of Samuel Adams*, 3: 83–84; 1: 59–60. For a traditional account of Adams’s role in precipitating the Tea Party, see, for example, Miller, *Sam Adams*, 294. It conflicts with a narrative in the Sewall Papers, Public Archives of Canada, Ottawa—“Proceedings of Ye Body Respecting the Tea”—which suggests that violence was detonated by an announcement that Governor Hutchinson had refused to issue a pass for the tea ships to leave Boston Harbor, and that Adams and his colleagues “called out to the People to stay” in the meeting despite the call of “hideous Yelling in the Street” because “they said they had not quite done.” The document, edited by L. F. S. Upton, is in the *William and Mary Quarterly*, 3d ser., 22 (1965): 297–98.

effectiveness. Redress, he understood, depended upon American strength, which depended upon internal unity, which was itself best achieved in what the people “easily see to be a constitutional opposition to tyranny.” Violence, by contrast, was divisive, and so Adams stressed not only the limits of its theoretical justifiability but also its political disutility. His advice to Rhode Islanders in 1773, that they prevent the Gaspée Commission from becoming the occasion of bloodshed, continued the following year in letters from Philadelphia to his besieged Boston colleagues. “Violence & Submission would at this time be equally fatal,” he wrote; and again, “Nothing can ruin us but our Violence.” He urged Joseph Warren “to implore every Friend in Boston by every thing dear and sacred to Men of Sense and Virtue to avoid Blood and Tumult” so as to “*give the other Provinces opportunity to think and resolve.*” When independence was finally declared, he was delighted that so important a revolution had been achieved “without great internal Tumults & violent Convulsions.”³²

His medium was not the mob but the press, the public celebration—like the Sons of Liberty dinner at Dorchester in August 1769, where some 350 patriots ate, saw a mimic show, and sang the “Liberty Song”—and, above all, the committee or association. This was true in 1772, when Adams believed tyranny was at hand in Massachusetts: “Let us . . . act like wise Men,” he counseled, and “calmly look around us and consider what is best to be done. Let us converse together. . . . Let every Town assemble. Let Associations & Combinations be everywhere set up to consult and recover our just Rights.” It remained true in 1776, when Adams complained that his compatriots were not doing enough to encourage enlistments in the American army. “Your Presses have been too long silent,” he scolded from Congress. “What are your Committees of Correspondence about? I hear nothing of circular Letters—of joynt Committees, &c. Such Methods have in times past raised [the] Spirits of the people—drawn off their Attention from *picking up Pins*, & directed their Views to great objects—.” Even his loyalist detractors testified to Adams’s skill as a writer, whether of legislative documents or for the press, and as an organizer. Joseph Galloway stressed his incredible energy as the leader of political factions both in Massachusetts and in Philadelphia; Peter Oliver mentioned that Adams had organized a singing society for Boston mechanics and somehow “embraced such Opportunities,” as Oliver saw it, “to ye inculcating Sedition.”³³

³² Adams in the *Independent Advertiser*, quoted in Wells, *Samuel Adams*, 1: 17; to Joseph Warren, Philadelphia, Sept. 1774; to Darius Sessions, Boston, Jan. 2, 1773; to Charles Thomson, Boston, May 30, 1774; and, on independence, to Benjamin Kent, Philadelphia, July 27, 1776, in *Writings of Samuel Adams*, 3: 157; 2: 398–99; 3: 124, 304; Adams to Joseph Warren, Philadelphia, misdated as May 21, 1774, in *The Warren-Adams Letters* (Boston, 1917–25), 1: 26.

³³ Adams as “Valerius Poplicola,” in *Boston Gazette*, Oct. 5, 1772, and to Joseph Warren, Philadelphia, May 12, 1776, in *Writings of Samuel Adams*, 2: 337; 3: 289–90; Hutchinson, *History of Massachusetts Bay*, 3: 212; Galloway, *Historical and Political Reflections*, 67–68; *Oliver’s Origin & Progress of the American Rebellion*, 41. See also, John Adams’s diary for Aug. 14, 1769, in *Works of John Adams*, 2: 218, on the Sons’ dinner: “This is cultivating the sensations of freedom. There was a large collection of good company. Otis and Adams are politic in promoting these festivals; for they tinge the minds of the people; they impregnate them with the sentiments of liberty; they render the people fond of their leaders in the cause, and averse and bitter against

All of this was radical enough for an age that could not yet accept nongovernmental political groups—parties, for example—as legitimate. Conventions and committees were often condemned as “extra-legal,” and so seditious, undistinguishable from common “mobs.” Within a few years the popular organizations of the resistance movement seemed of questionable acceptability even to Samuel Adams. He never regretted his participation in those of earlier days; indeed, they had served “an excellent purpose” then in facilitating public watchfulness over those in authority. But with the establishment of regular, constitutional, republican government, under which all men in authority depended upon free, annual elections, committees and conventions were “not only useless, but dangerous.” Decency and respect were due constitutional authority; bodies of men who convened to deliberate and adopt measures cognizable by legislatures might bring legislatures into contempt and “lessen the Weight of Government lawfully exercised.”³⁴ And so he opposed all popular threats to republican government. He served in 1782 on a legislative committee to visit Hampshire County and “inquire into the grounds of disaffection,” to quiet any “misinformations” and “groundless jealousies” that lay behind local insurrections. Four years later he acquiesced fully in measures to suppress Shays’ Rebellion, which he considered a Tory effort to undermine the Revolution. He may even have argued, as one memorialist claimed, that “in monarchies the crime of treason and rebellion may admit of being pardoned or lightly punished; but the man who dares to rebel against the laws of a republic ought to suffer death.” He was no less definite in condemning Pennsylvania’s “Whisky Rebels,” who rose against the federal excise tax in 1794. “No people can be more free [than] under a Constitution established by their own voluntary compact, and exercised by men appointed by their own frequent suffrages,” Governor Adams told the Massachusetts legislature. “What excuse then can there be for forcible opposition to the laws? If any law shall prove oppressive in its operation, the future deliberations of a freely elected Representative, will prove a constitutional remedy.”³⁵

To James Warren, an old revolutionary who opposed the suppression of Shays’ Rebellion, Samuel Adams seemed to have forsaken his old principles, “to have become the most arbitrary and despotic Man in the Commonwealth.”³⁶ There were, however, deep continuities in Adams’s attitudes. Always he fought as the defender of the free constitutional government of Massachusetts, whether against Hutchinson, Britain, or western insurgents. Before 1776 he justified the resort to popular meetings and direct

all opposers.” See also entry for Sept. 3, 1769, in *ibid.*, 219: “supped with Mr. Otis, in company with Mr. Adams. Mr. William Davis, and Mr. John Gill. The Evening spent in preparing the next day’s newspaper,—a curious employment, cooking up paragraphs, articles, occurrence, &c., working the political engine!”

³⁴ Samuel to John Adams, Apr. 16, 1784, and to Noah Webster, Apr. 30, 1784, in *Writings of Samuel Adams*, 4: 296, 305–06.

³⁵ Wells, *Samuel Adams*, 3: 162, 246; Adams to legislature, Jan. 16, 1795, in *Writings of Samuel Adams*, 4: 373.

³⁶ James Warren to John Adams, Milton, May 18, 1787, in *Warren-Adams Letters*, 2: 292–93.

force only on rare occasions when all alternatives failed. With the foundation of the republic such occasions evaporated altogether. Henceforth even the most severe threats of power to freedom and constitutional rule, such as had prompted the English Revolution of 1688 and the American Revolution of 1776, could be brought down through established, lawful procedures, as would be done in the “Revolution of 1800” and that of 1974. Only in countries like France, where the republic had yet to be established, could the older type of revolution, with its popular associations and mass uprisings, be justly continued.

ALL OF THIS IS AT SUCH ODDS with the stereotypic Samuel Adams that it raises a final question: why should written history and historical evidence be so contradictory? At least three explanations are possible. One is concerned with historians’ use of documentation, another more generally with the way Americans have related to their Revolutionary tradition. A third centers on Samuel Adams as a historic person.

Biographers have, in the first place, consciously dismissed Adams’s writings as an unreliable historical source. His papers, Shipton charged, were censored both by Adams and, after his death, by John Avery. John Adams did leave a graphic picture of Samuel in Philadelphia destroying whole bundles of his papers lest they fall into the hands of the British and be used against his correspondents. The substantial collection of writings Samuel nonetheless left at his death was diminished, it seems, largely through neglect, although a hint of conscious censorship remains in William V. Wells’s remark that “there is . . . reason to believe that letters were abstracted early in the present century by persons interested in their Suppression.”³⁷ Historians can, however, work with incomplete manuscripts, balancing biases and supplementing lapses with other sources of evidence—unless the whole must be dismissed as deceitful in character, the work of a man whose consistent technique, as Shipton claimed, was that of “the lie reiterated.” That charge echoes through modern biographies: Hosmer first agreed with those loyalists who found “great duplicity” in Adams’s conduct; Miller found his “sincerity open to question”; for Harlow, Adams’s writings were but the “psychopathic effusions” of a man who “evaded the truth, and mishandled the facts so glaringly that almost everything he wrote is a demand for refutation.” This conclusion stems most often from a conflict between the biographers’ unquestioned assumption that Adams was long dedicated to independence and the testimony of his writings, which authors resolve by rejecting the latter. As Harlow put it, Adams “pretended to be a peace-loving colonist, desiring nothing so much as peace and quiet” only to “veil his real aggressions upon British authority.”³⁸

³⁷ Shipton, “Samuel Adams,” 444; John Adams to William Tudor, Quincy, June 5, 1817, in *Works of John Adams*, 10: 264; Wells, *Samuel Adams*, 1: x, xi.

³⁸ Shipton, “Samuel Adams,” 444; Hosmer, *Samuel Adams*, 120–21; Miller, *Sam Adams*, 228–29; Harlow, *Samuel Adams*, 190, 357, 87–88. The charge of dishonesty is particularly complex. By modern lights many of

The modern image of Samuel Adams stems, however, not only from historians' suspicions of Adams in particular, but also from a broader ambivalence toward the earliest days of the Revolution. With the establishment of the republic came a rejection of extralegal opposition to authority. As Adams himself fully understood, a continuation of resistance as established before 1776 imperiled the successful conclusion of America's experiment in popular self-government. Yet the Revolution remained the one common, identifying experience of Americans; if cleansed of its anarchistic implications, it could serve as a powerful symbol to counteract the forces of disintegration and help establish the new nation. And so the Revolution was subtly transformed into the war for independence, a more suitable heroic rallying point than the fundamental reformation that revolution implies. Meanwhile, the "old revolutionaries," leaders of the resistance to Britain, were gradually confounded with the Founding Fathers of later years, who were then sanctified—as by Mason Weems, whose superhuman Washington symbolized the Revolution for generations of American school children. Persons whose importance was confined to the period before 1776—not "secondary figures of the Revolution" but primary figures of a first stage of the Revolution such as Christopher Gadsden, Isaac Sears, and Cornelius Harnett—were forgotten or, where their prominence precluded obfuscation, mythologized over time into symbols of all that had to be rejected in the Revolution.

The Adams myth had its roots, then, in the earliest decades of the new nation. But it appropriately took modern form during the 1880s in the hands of James Hosmer who, like other historians of his time, regretted the division of English peoples that 1776 had entailed. Sympathy then, as now, went naturally toward the loyalists who found more to fear in "the breaking down of the old system" than in submission to Parliament, "honest men" who, as Shipton said, were forced to flee from "the unreasoning rage of people among whom their families had lived as friends and public servants for generations." Behind these sympathies there remains a rejection in American life of what the Federalists called "Jacobinism." Harlow asked the critical question:

the issues that inspired fears in the eighteenth century seem benign, and so, unless biographers consider the revolutionaries' distinctive ideological assumptions, Adams's treatment of events, his "persistence in attributing evil motives to those men he fought," seems not just mistaken but dishonest. See biographies by Harlow, pp. 357–58, and Shipton, p. 444. Shipton also claimed that "a comparison of the letters which Adams wrote to those of his friends who knew what was going on in Boston with those written to friends who were not in a position to know the truth will show that he was not simply the victim of blind prejudice" (p. 426). No specific letters were cited, however, and Adams's printed letters are not contradictory. On independence, for example, Adams's position was remarkably consistent in any one time period, regardless of his correspondent. Perhaps his most revealing letters were in fact to his Boston colleagues from Philadelphia. The original loyalist charge that Adams would stoop to anything to serve his cause stemmed, I suspect, from his political maneuvers—from the ruse he used to allow the publication of Hutchinson's letters, for example, or the deceptions he practiced so as to exclude the loyalist Daniel Leonard from certain critical legislative deliberations. On the latter, see Robert Treat Paine's "Account of Stratagem . . .," in Paine Papers, Massachusetts Historical Society, Boston. I am grateful to Jack N. Rakove for the last reference. The loyalists' charges of Adams's fallaciousness came also from their disagreement with his political stands. See Hutchinson, *History of Massachusetts Bay*, 3: 212. To a considerable extent, then, the charge of dishonesty is incapable of direct, "objective" refutation or verification. It reflects the observer's politics more than the subject's morality.

"How many of the vociferously 'loyal' Americans of today, those staunch enemies of twentieth century radicalism, would have looked with favor upon rebellion against established authority in 1775. The latter-day patriots profess great admiration for the 'fathers' of the Revolution, but the real test is to be found . . . in the attitude toward the *spirit* of revolution today." As that attitude became increasingly negative, Samuel Adams was rejected with increasing vehemence.³⁹

There remains, however, a more complex explanation for the discrepancy between modern biographers' interpretations of Samuel Adams and historical evidence. Behind all the rejections of Adams, all the accusations of deceit, lies a profound problem of relating not just to the Revolution as a tradition, but to the revolutionary as a type, and so to Adams as a historic person. For the characteristics that distinguish him from most other men include an ascetic civil commitment that at once accounts for his democratic inclinations, confounds modern observers, and links him with revolutionaries of other times and places.

At first destined for the clergy, politics instead became Adams's ministry. He was one of the first Americans willing to identify himself as a politician⁴⁰—which made him distinctly modern—but only because he understood that role as akin to a religious vocation: there was great moral content in the cause of "Liberty and Truth," as he once called it. Virtue was the most emphatic theme of his writings and of his life. It implied austerity, a "sobriety of manners, . . . Temperance, Frugality, Fortitude," but above all a willingness to sacrifice private advantage for the cause of the community, to subject the self to a greater cause. Only a "virtuous people" could "deserve and enjoy" freedom. Should they become "universally vicious and debauched" they would, whatever the form of their institutions, become "the most abject slaves."⁴¹ A man who held such a creed so emphatically was less suited for the role of Founding Father than of moral reformer. Just as he had condemned the corruption and dissipation of Englishmen, Adams railed at the "Luxury and Extravagance" of Boston in 1778, fearing it would be "totally destructive of those Virtues which are necessary for the Preservation of the Liberty and Happiness of the People." He called for reformation and labored to keep the theater, that cauldron of dissipation, out of Boston. The patriot was of course a virtuous man: he worked for the cause selflessly. "It would be the glory of this Age, to find Men having no ruling Passion but the Love of their Country, and ready to render her the most arduous and important Services with the Hope of no other Reward in this Life than the Esteem of their virtuous Fellow

³⁹ Shipton, "Samuel Adams," 428; Harlow, *Samuel Adams*, 265–66. On the simultaneous rejection of the Revolution and revival of loyalist studies in the late nineteenth century, see Bailyn, *Ordeal of Thomas Hutchinson*, 393–403. The contemporary political basis of this historical view is often clearer in popular histories. See, for example, Stewart H. Holbrook's *Lost Men of American History* (New York, 1946), 11–32, especially 22–23, where he dismisses local resistance leaders as "all left-wingers in their respective regions" who circulated "the Adams brand of poison."

⁴⁰ As, for example, in a letter to Samuel Cooper, Philadelphia, Dec. 25, 1778, in *Writings of Samuel Adams*, 4: 107.

⁴¹ Adams to Samuel P. Savage, Philadelphia, Oct. 6, 1778, in *ibid.*, 4: 67–68; essay in *Independent Advertiser*, 1749, in Wells, *Samuel Adams*, 1: 23.

Citizens," he wrote in 1778. "But this, some tell me, is expecting more than it is in the Power of human Nature to give."⁴²

Yet he himself lived this unlikely creed, privately as well as publicly, becoming the embodiment of republicanism, a man who contained, as Brissot de Warville observed, "the excess of Republican virtues, untainted probity, simplicity, modesty, and, above all, firmness." He eschewed wealth. Eliot described him as "a poor man, who despised riches, and possessed as proud a spirit as those who roll in affluence or command armies." When he first went to the Continental Congress, his friends felt compelled to buy him proper clothes, that he might be outfitted respectably. On his retirement from the governorship and public office at the age of seventy-four, Adams justly proclaimed that he had not been enriched in the public service. His main support in old age came, it seems, from his only son, who died in 1788, leaving to his father a set of claims upon the United States for services as a surgeon during the Revolutionary War.⁴³

Samuel Adams also eschewed personal glory. Here, as in the character of his talents and his later politics, he contrasted dramatically with John Adams, who early in the Anglo-American conflict reflected upon the opportunity his times afforded a young man aspiring for fame like that of the Hampdens and Sidneys of ages past, and who scrupulously preserved his own papers, dogging Samuel to do the same. But Samuel was less concerned about his manuscripts or his place in history: "I do not keep copies of all my letters," he once wrote, "—they are trifles."⁴⁴ In this life, too, he was less ambitious than John. He never sought prominent executive office until his later years, contenting himself with positions in representative bodies or committees, with those behind-the-scenes tasks that brought political effectiveness, and suspicion, but not necessarily prominence. Nor was power a consolation; Adams did not always determine the arguments or programs of the patriots but instead was "exclusively entitled to the merit of connecting them into one system, and infusing into the scattered efforts of many, all the life and energy which belongs to a single will." Inner rectitude was what he sought. "If my mind has ever been tainted with Envy," he wrote his wife, "the Rich and the Great have not been its objects. . . . He who gains the Approbation of the Virtuous Citizens . . . may feel himself happy; but he is in Reality much more so, *who knows he deserves it*. Such a Man, if he cannot retreat with Splendor, he may with dignity."⁴⁵

⁴² Adams to Savage, Philadelphia, Oct. 6, 1778, and to James Warren, Philadelphia, July 1778, in *Writings of Samuel Adams*, 4: 67–68, 46. On the theater controversy, see Wells, *Samuel Adams*, 3: 290–91, and S. E. Morison, "Two 'Signers' on Salaries and the Stage, 1789," *Proceedings of the Massachusetts Historical Society*, 62 (Boston, 1930): 55–63.

⁴³ J. P. Brissot de Warville, *New Travels in the United States of America* (Boston, 1797), 65; Eliot, *Biographical Dictionary*, 16; Adams to legislature, Jan. 27, 1797, in *Writings of Samuel Adams*, 4: 404; Wells, *Samuel Adams*, 2: 207–09; 3: 332–33.

⁴⁴ John Adams, "Dissertation on Canon and Feudal Law" (1765), in *Works of John Adams*, 3: 463; John to Samuel Adams, Paris, Apr. 5, 1783, Samuel Adams Papers, New York Public Library, microfilm roll 5; Adams to Savage, Philadelphia, Nov. 1, 1778, in *Writings of Samuel Adams*, 4: 87.

⁴⁵ Charles Francis Adams, "Life of John Adams," in *The Works of John Adams*, 1: 124; Adams to Elizabeth Adams, Philadelphia, Nov. 24, 1786, in *Writings of Samuel Adams*, 4: 226. Hutchinson noted how much more ambitious John Adams was than Samuel. See his *History of Massachusetts Bay*, 3: 214. On Adams's contribution, see also Brown, *Revolutionary Politics*, 54n.

Such statements suggest Adams, like Benjamin Franklin, may have mastered whole catalogs of virtues only to stumble on the sin of pride. Yet humility suffused his life. He came from a respectable family. In a period when Harvard students were numbered according to social position, Samuel was sixth in the class of 1740 and, as Shipton notes, had the ordinary rules been followed that year, he would have been second. But, as John Eliot recalled, "Every kind of genealogy he affected to despise, as a thing which gives birth to family pride." He had, moreover, the rare ability to recognize, as did others, that he was "not a man of ready powers," that his strengths were limited. And so he recruited others for roles he could not fulfill.⁴⁶ Nor did Samuel cherish notions either of his own peculiar importance or of the public's obligation to him. John Adams left office in 1801 with great bitterness, but Samuel, in similar circumstances twenty years earlier, reminded his indignant friends "that in a free Republic, the People have an uncontrollable right of chusing whom they please" for public offices. No man, he said, had a claim on his country for having served it, for that was simply a citizen's duty. Again on his final retirement from politics, Adams affirmed a long-standing conviction that others more able could take his place.⁴⁷ This refusal to cultivate or elevate the self shaped his personality. He watched himself as closely as he counseled the people to observe their rulers and learned to control a natural passion and temper much as he led his countrymen to eschew violence in the name of a larger good. "If Otis was Martin Luther . . . [who was] rough, hasty and loved good cheer," John Adams remarked, Samuel Adams was John Calvin, "cool, abstemious, polished, refined, though more inflexible, uniform, consistent."⁴⁸

Having denied himself special significance, he naturally respected others who had still fewer traditional claims to status. "No man ever despised more those fools of fortune, whom the multitude admire" than did Adams; "and yet," Eliot noted, "he thought the opinion of the common people in most cases to be very correct." He was "well acquainted with every shipwright, and substantial mechanick, and they were his firm friends through all the scenes of the revolution, believing that to him more than any other man in the community we owed our independence." His writings often took on the guise of speaking for the people; and, though he emphasized the theme of equality mainly in the 1790s when inspired by the French Revolution, he saw it as a major purpose of civil society already in 1771. Yet he was not, as he once put it, of levelling principles. Subordination was necessary for government; public rank was to be respected, and rigidly.⁴⁹ Status must, however, be earned.

⁴⁶ Shipton, "Samuel Adams," 420; Eliot, *Biographical Dictionary*, 5-7; Bentley, diary, Oct. 3, 1803, in *Diary of William Bentley*, 3: 49; John Adams to William Tudor, Quincy, Feb. 9, 1819, in *Works of John Adams*, 10:364-65.

⁴⁷ Adams to Elizabeth Adams, Philadelphia, Nov. 24, 1780; to Caleb Davis, Philadelphia, Apr. 3, 1781; and to the legislature, Jan. 27, 1797, in *Writings of Samuel Adams*, 4: 226, 254-55, 404.

⁴⁸ John Adams to J. Morse, Quincy, Dec. 5, 1815, in *Works of John Adams*, 10: 190.

⁴⁹ Eliot, *Biographical Dictionary*, 5-7; Adams to legislature, Jan. 17, 1794, and as "Vindex," in *Boston Gazette*, Jan. 21, 1771, in *Writings of Samuel Adams*, 4: 357-59; 2: 151-52. For the rigidity with which he came to believe elective public office should be respected, see James T. Austin, *Life of Elbridge Gerry* (Boston, 1828-29), 1: 474.

Samuel Adams's politics, Bentley claimed, came from "two maxims, rulers should have little, the people much. The rank of rulers is from the good they do, & the difference among the people only from personal virtue. No entailments, no privileges. An open world for genius & industry." Even wealth was an inappropriate, and indeed dangerous, criterion for public office. It would be better, Adams once argued, to prefer men in want over those with riches, for while the former could be done "from the feelings of humanity, . . . the other argues a base, degenerate, servile temper of mind." Ability and virtue were alone appropriate qualifications.⁵⁰

This unease with preordained rank permeated even his marriage. Not that his theory of familial relationships was anything but traditional, even Biblical: "It is acknowledged," he wrote his future son-in-law, "that the Superiority is & ought to be in the Man." But since "the Management of a Family in many Instances necessarily devolves on the Woman, it is difficult always to determine the Line between the Authority of the one & the Subordination of the other." Standard notions of the proper concerns of the sexes he found equally difficult to maintain. His letters to his wife from Philadelphia are filled with public affairs, to the point of humoriness. Tell Samuel Cooper, who wanted political news, "that I can scarce find time to write you even a Love Letter," he once wrote her. "I will however for once give you a political Anecdote." Occasionally this practice made him uneasy: "I forget that I am writing [to] a female upon the Subject of War," he once apologized, and, again, explained that he sent along so much political news because his letters to her were also meant for his male colleagues in Boston. But ultimately no apologies were necessary for, he recalled, her "whole Soul" was "engagd in the great Cause." It seemed appropriate to him that, with the Revolution, Boston should consider improvements in the education of female children. And as for the family, it seemed best "not to govern too much." His private inclinations had, moreover, public implications, for there was great uniformity, he suggested, in the practices that promoted stability for states, cities, and families.⁵¹

Adams's personal achievement of virtue shaped his writings, which explains in part why one of the most prolific of Revolutionary authors has been the least understood. By the dominant canons of the Revolutionary period not only politics but even literature was to be selfless: authors must not be themselves revealed or advanced, but only their causes. Samuel Adams's public writings are within this tradition. Even his family correspondence is remarkably impersonal. He found it difficult to lay aside a rhetorical stance: "Why do I write in this Stile," he once wondered to his cousin John. Occasional letters to his daughter resemble nothing so much as replies from the Boston Committee of Correspondence. On September 8, 1778, for

⁵⁰ Bentley, diary, Oct. 3, 1803, in *Diary of William Bentley*, 3: 49; Adams to Elbridge Gerry, Philadelphia, Jan. 2, 1776, in *Writings of Samuel Adams*, 3: 247.

⁵¹ Adams to Thomas Wells, Philadelphia, Nov. 22, 1780; to Elizabeth Adams, Philadelphia, Feb. 26, 1776, Nov. 14, 1776, Apr. 1, 1777, and Aug. 8, 1777; to John Adams, Boston, Dec. 8 and 19, 1781; and to John Scollay, Philadelphia, Dec. 30, 1780, in *Writings of Samuel Adams*, 4: 224; 3: 267, 319, 367, 403-04; 4: 270, 237.

example, he quoted back passages from the girl's own previous letter, which informed him of his wife's illness, drawing inferences, approving proper sentiments: "I am satisfied 'you do all in your power for so excellent a mother.' You are under great obligations to her, and I am sure you are of a grateful disposition. I hope her life will be spared, and that you will have the opportunity of presenting to her my warmest respects." Always the cause, or lesson, is primary, whether filial duty or, as in a letter of 1780, religiosity. At most he adds personal assurances "that I have all the feelings of a father," then signs—"S. Adams." Ultimately, of course, these letters are revealing of their author. They confirm an observation by the marquis de Chastellux, so like that of Brissot, that in Samuel Adams one could experience "the satisfaction one rarely has in society, or even at the theatre, of finding the person of the actor corresponding to the role he plays." His "simple and frugal exterior" and his conversation—like his writings—were all of a piece.⁵²

An eighteenth-century Frenchman could, then, understand Samuel Adams. It seems unlikely, moreover, that Adams was so enigmatic to his Revolutionary colleagues as William Bentley claimed. His confidences were limited to a handful of men,⁵³ but with these he found it painful to keep secrets even when communications were by post and longed for those private conversations where like-minded men could "disclose each others Hearts." Among these friends Adams won—his correspondence occasionally reveals—affection even for his idiosyncracies. In December 1777 he visited Plymouth to help celebrate the anniversary of the Pilgrims' landing and sent James Lovell a report that stressed above all the merits of the day's sermon. "An Epicure would have said something about the clams," Lovell replied, "but you turn me to the prophet Isaiah."⁵⁴ By the early nineteenth century, however, Samuel Adams seemed a remote figure, "one of Plutarch's men. Modern times have produced no character like his that I can call to mind," one clergyman commented. For John Adams, an analogy with Calvin was apt. But for a still more distant, less religious age, one more concerned with the "inner springs" of conduct, more insistent that writers produce advertisements of themselves, not of their causes, Samuel Adams awoke only suspicion. "He was working for liberty," Harlow wrote, "but why does anybody devote a life to an abstract cause? Conscious motives will no more explain Samuel Adams than they will explain Mohammed, Peter the Hermit, Savonarola, or Joan of Arc."⁵⁵

The consistent citation of religious analogues is appropriate, for Adams was, in short, a saint, "the last of the Puritans" as Edward Everett called him. His Puritanism was internalized, in the formation of his character, and sec-

⁵² Samuel to John Adams, Boston, Sept. 16, 1776, in *ibid.*, 3: 313; letters to his daughter Hannah, in Wells, *Samuel Adams*, 3: 53–54; de Chastellux, *Travels in North America*, 1: 142.

⁵³ Cf. Adams to Darius Sessions, Boston, Dec. 28, 1772, and to John Adams, Feb. or Mar. 1773, in *Writings of Samuel Adams*, 2: 289, 430.

⁵⁴ Adams to Joseph Warren, Philadelphia, Nov. 4, 1775, and to Samuel Cooper, Philadelphia, Apr. 23, 1781, in *ibid.*, 3: 234; 4: 259; Lovell to Adams, Jan. 20, 1778, Samuel Adams Papers, microfilm roll 5.

⁵⁵ Clergyman quoted in Wells, *Samuel Adams*, 2: 185; Harlow, *Samuel Adams*, 64.

ularized into political doctrine. As such, although he remained a devout congregationalist, Adams was less a religious sectarian than, in a term he once used, a “political Enthusiast.” His politics were consciously modeled upon those of New England’s founders. Few men have been so conscious of their place in time, so capable of deriving meaning from it. Ancestors and posterity rank only with virtue as key concepts in his writings, and all were linked in a conception that cited past and future to define the duties of the present. In short, for Adams the past was a property that gave him identity, direction. The freedom of Massachusetts was inherited from ancestors who left England “to settle a plan of govt upon the true principles of Liberty.” New England’s fathers left to their children not just the institutions, but the habits of freedom: “Our Bradfords, Winslows & Winthrops would have revolted at the Idea of . . . Dissipation & Folly,” Adams wrote, “knowing them to be inconsistent with their great Design, in transplanting themselves into what they called this ‘Outside of the World.’” There was, then, an obligation upon the living to make “every laudable Effort” to continue the ways of the fathers, to secure for posterity “the free & full Enjoyment of those precious Rights and privileges for which our renowned forefathers expended so much Treasure and Blood.”⁵⁶

Thus Adams’s role was that of an intermediary, passing the achievements of the past on to the future. He was not breaking new paths, discovering new worlds, but traveling a well-marked highway, which accounts for his confidence and rectitude, the “sternness of stuff” upon which John Adams commented. It was simply his duty “to oppose to the utmost of my Ability the Designs of those who would enslave my Country; and with Gods Assistance I am resolved to oppose them till their Designs are defeated or I am called to quit the Stage of Life.”⁵⁷ Because of this self-conception, Adams’s conflict with Thomas Hutchinson was critical in his development as a revolutionary. Hutchinson was to Adams a son of New England—“bone of our Bone, & flesh of our flesh”—who turned on his native land, a man who would “aid the Designs of despotick power” in his willingness to compromise the colony’s charter, even to recommend an abridgment of English liberties in America, all to satisfy his ambition for power and wealth.⁵⁸ And because he was so successful in attaining positions of influence, Hutchinson threatened the continuity of past and future to which Adams was so dedicated.

⁵⁶ Everett, “The Battle of Lexington,” Apr. 19, 1835, in his *Orations and Speeches* (Boston, 1850–68), 1: 545; Adams to Arthur Lee, Boston, Apr. 9, 1773, and Sept. 27, 1771; Committee of Correspondence to Thomas Mighill, Boston, Oct. 7, 1773; Adams to John Scollay, Philadelphia, Dec. 30, 1780; and to Joseph Warren, Philadelphia, Feb. 12, 1779, in *Writings of Samuel Adams*, 3: 21; 2: 236; 3: 18; 4: 238, 124–25. For an earlier and different effort to link Adams’s politics with his Calvinism, see Williams, “Samuel Adams,” especially 48–50. Williams emphasizes above all Adams’s dedication to “a corporate Christian commonwealth supported by the political economy of mercantilism” (p. 57). W. E. H. Lecky was more moved by Adams’s character in his very brief description of the Massachusetts leader as a “seventeenth-century Covenanter.” See *The American Revolution, 1763–1783* (New York, 1922), 120.

⁵⁷ Adams to Elizabeth Adams, Baltimore, Jan. 29, 1777, in *Writings of Samuel Adams*, 3: 349.

⁵⁸ Adams to Stephen Sayre, Boston, Nov. 23, 1770; to John Scollay, Philadelphia, Dec. 30, 1780; to Elizabeth Adams, Philadelphia, Mar. 23, 1779; and as “Valerius Poplicola” and “Candidus,” in *Boston Gazette*, Oct. 28 and Nov. 25, 1771, in *ibid.*, 2: 67–68; 4: 237–38; 4: 138; 2: 256–64, 268–76. See also Bailyn, *Ordeal of Thomas Hutchinson*, 183–84.

From these local origins, Adams's radicalism spread until he opposed not only England, whose politics shaped and sustained Hutchinson, but also the old order throughout Europe. In his effort to preserve his vision of a pure and virtuous past, Adams was, like other revolutionaries, pushed increasingly toward innovation.⁵⁹ His earlier equalitarian inclinations evolved into an articulate republicanism, until by 1785 Adams found all hereditary rule, indeed all institutions save those of a republic, "unnatural," tending "more or less to distress human Societies." Since the American Republic threatened all traditional establishments, Adams thought it would naturally evoke efforts at repression. "Will the Lion ever associate with the Lamb or the Leopard with the Kid," he asked Richard Henry Lee, "till our favorite principles shall be universally established?"⁶⁰ Yet even revolutionary republicanism constituted only a revival of his ancestors' ways. Was the unpretentiousness of those early delegates to the Continental Congress—who, Adams recalled with approval, ate simple lunches of bread and cheese under a tree—so different from that of early New Englanders who were "contented with Clams and Muscles [*sic*]"? It was, he claimed, the "Principles and Manners of N[ew] Eng[lan]d" which "produced that Spirit which finally has established the Independence of America." And "the genuine Principles of New England," he suggested, were quite simply "Republican Principles."⁶¹

In the end, however, Adams is more usefully compared with England's seventeenth-century revolutionary saints than with New England's founders, who retreated to a wilderness to found their commonwealth. Like Adams, Britain's revolutionary Puritans were engaged, as Michael Walzer notes, in a lay ministry within a corrupted world; like him, they cited a purer past against the sinful present. More fundamentally, Adams followed his English political ancestors in his espousal of godly vigilance, playing the role of "watchman on the wall," alerting his city of new threats to its freedom; in his exercise of magistracy—creating popular associations, which marked a break with the more feudal past in America as in England, opening the way toward modern politics; devoting endless hours to committee work both in Boston and at the Continental Congress—and, ultimately, in his endorsement of revolution. His severe asceticism, moreover, links Adams not only with earlier Puritan revolutionaries but with Jacobins of the Republic of Virtue, Bolsheviks of the early days of the Russian Revolution, and the Chinese in Yenan.⁶² The myth of Samuel Adams may then be wrong not just in misconceiving his personal identity, but also in its more fundamental

⁵⁹ On the conservative beginnings of revolutionary movements, see Hannah Arendt, *On Revolution* (New York, 1965), 34–40, 153.

⁶⁰ Adams to R. H. Lee, Boston, Apr. 14, 1785, in *Writings of Samuel Adams*, 4: 238, 408–11.

⁶¹ Adams to James Warren, Yorktown, May 25, 1778, in *Warren-Adams Letters*, 2: 13; to William Checkley, Boston, June 1, 1774; to James Warren, Philadelphia, Feb. 12, 1779; and to Samuel Cooper, Philadelphia, Apr. 23, 1781, in *Writings of Samuel Adams*, 3: 128; 4: 124, 259–60.

⁶² Michael Walzer, *The Revolution of the Saints: A Study in the Origins of Radical Politics* (Cambridge, Mass., 1965), 3–4, 12–13, 28–29, 204–10, 267, 306, 310–20. For a further example of revolutionary asceticism, among Vietcong revolutionaries in the Mekong Delta, see David Hunt, "Villagers at War: The National Liberation Front in My Tho Province, 1965–1967," *Radical America*, 8 (1974): 22–24, *passim*.

assumptions about the character of a revolutionary. Successful revolutionary leaders are not violent and irresponsible anarchists but persons of intense discipline and policy for whom the public cause purges mundane considerations of self.

Men such as these are destined to be misunderstood by later generations, for they play transitional roles in their revolutions. "They . . . helped carry men through a time of change" but "had no place in a time of stability." With the consolidation of the new order their acute Spartanism becomes uncomfortable, outmoded. The experience of several American "old revolutionaries" was here strikingly like that of an old Red Army man portrayed in a poem of 1924 by Sergei Esenin: "What a misfit I've become," he says after nostalgically recalling victories of the Russian Revolution; "I feel a foreigner in my land."⁶³ Similarly, James Warren made a sentimental pilgrimage to Concord in 1792 but found "few of the old hands, and little of that noble spirit, and as little of those comprehensive views and sentiments which dignified the early days of the revolution. Thus," he concluded, "I have lived long enough to feel pains too great for me to describe." And by March 1801 South Carolina's Christopher Gadsden found the new world for which he had helped prepare the way a "mere bedlam."⁶⁴

Samuel Adams never succumbed to such acute disillusionment. He questioned whether Boston, much less America, would ever become a "*Christian Sparta*" as he once hoped, but he never lost his deeper faith in the people, never despaired of the Revolution. The nation's rejection of John Adams in 1800, which so disturbed Gadsden, was for Samuel Adams the end of a brief nightmare of armies and repression. "The storm is now over," he wrote Thomas Jefferson in 1801, "and we are in port," with peace and harmony in store. The principles of democratic republicanism were better understood than ever before, and, "by the continued efforts of Men of Science and Virtue," there was reason to believe they would "extend more and more till the turbulent and destructive Spirit of War shall cease," and "principles of Liberty and virtue, truth and justice" might "pervade the whole Earth."⁶⁵

For younger Americans, Adams appeared nonetheless intolerant, austere, inflexible, a man buried in his Puritan past. At best he was a person "born for the revolutionary epoch," one who "belonged to the revolution."⁶⁶ In either case, Adams seemed out of place in later times, an alien. Discomfort contributed to the growth of myth, which further separated future generations from the historic man. It also testified to Samuel Adams's quintessentially revolutionary character, and so to the genuinely revolutionary character of the American Revolution.

⁶³ Walzer, *Revolution of the Saints*, 320; Esenin, "Soviet Russia," in *ibid.*

⁶⁴ James Warren to Gerry, Plymouth, Mass., Dec. 18, 1792, in *A Study of Dissent: The Warren-Gerry Correspondence*, ed. C. Harvey Gardiner (Carbondale, Ill., 1968), 251; Gadsden, *The Writings of Christopher Gadsden, 1746-1805*, ed. Richard Walsh (Columbia, 1966), 306-07.

⁶⁵ Adams to John Scollay, Philadelphia, Dec. 30, 1780, and to Jefferson, Apr. 24 and Nov. 18, 1801, in *Writings of Samuel Adams*, 4: 238, 408-11.

⁶⁶ Tudor, *The Life of James Otis*, 275.

The Last Viceroy of New Spain and Peru: An Appraisal

TIMOTHY E. ANNA

THE BIBLIOGRAPHY OF THE Spanish American Wars of Independence (1810–24) is, to understate the obvious, vast. It is also, and this has apparently not been so obvious, one sided. It concentrates almost entirely on the rebels, on the American side, on their aspirations, their objectives, and their military and political campaigns. The literature on the royalist side is very incomplete. There are, of course, many valuable studies in what might be called the “background” to the movements for independence, studies concentrating on the eighteenth century and on the progressive decline of Spain’s world system, on what was wrong with that system and why Americans perceived it to be unsuited to them. This background material, however, leaves a major gap in our understanding, for, although it tells us what Spanish Americans themselves thought to be grievances in the imperial system, it does not, as is sometimes assumed, automatically tell us how that system collapsed. Much less does it tell us what the royalists were thinking, or their objectives in the war itself, or the mistakes they made in the war. Given the fact that, as Hugh M. Hamill, Jr. has pointed out in the case of Mexico, the majority of Spanish Americans were not decided on the question of independence,¹ exclusive concentration on the fundamental weaknesses of the royal system and on American objections to it, important though these were, does not tell the whole story. It may explain, for example, why the royal system “deserved” to be overthrown or why Americans perceived it to deserve that fate, but it does not explain how it was overthrown. Similarly, a thousand studies of military campaigns only tell how battles were won and lost. One might even go so far as to say that a thousand biographies of Simón Bolívar, José de San Martín, Miguel Hidalgo, José María Morelos, and Agustín de Iturbide only tell how they won, not how Spain lost. As C. H. Haring long ago pointed out, Spain’s imperial system in America may not have been the world’s best government, but it was not the world’s worst.² It may not have made room for or fulfilled the aspirations of Americans, but it was no mere house of cards.

It is well past time, then, that Latin American historians specializing in the

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¹ Hugh M. Hamill, Jr., *The Hidalgo Revolt: Prelude to Mexican Independence* (Gainesville, 1966), 151.

² C. H. Haring, *The Spanish Empire in America* (New York, 1947), 113.

emancipation focus a proportionate amount of attention on the royalists. Furthermore, that focus should be concentrated, not in the eighteenth century, much less in the sixteenth, not at the time Spain's imperial institutions were created, or even at the moment of their most important institutional reform under the Bourbons, but during the Wars of Independence.³ No matter how widespread the radical ideas of the French and North American philosophers were, no matter how corroded Spain's ability to govern may have been, the obvious and automatic answer to what Americans saw wrong in the empire was not independence. It would have been, rather, reform, accommodation, and compromise. Even when the uprisings actually began, as, for example, in the case of the Hidalgo insurrection, independence was not the logical or automatic objective. It was not until 1813 in Mexico, 1816 in Río de la Plata, and 1821 in Lima that rebels formally proclaimed independence. Something must have got in the way, something must have acted after 1810 to convert the insurgent cry of "Death to bad government" into "Long live independence."

One of the fundamental mistakes the historian could make in reviewing the royal government during the Wars of Independence would be to think of it as functioning the way the law required it to. It did not, because it could not. Three centuries of restrictions and controls over the exercise of the Crown's power by its agents in America had not prepared them for the catastrophe of 1808. During most of the Wars of Independence the royal power in America functioned virtually on its own, because from 1808 to 1814 the monarch was a captive in France and from 1820 to 1823 he was a captive of the liberal Spanish Constitution. Thus, while in theory major policy decisions came exclusively from Spain—and many of them did—in practice throughout this era an extraordinary amount of major policy was made by the viceroys, and almost in spite of their natural absolutist inclinations. This is not to deny that the intransigence of peninsular Spain on the question of American autonomy also played a great role in provoking American desires for total separation. But the focus should first be on Lima and Mexico City.

THE THESIS OF THIS ARTICLE is that the viceroys themselves, unwittingly and without recognizing it, disproved the myths upon which Spanish imperial absolutism was based. To put it another way, they proved Spanish imperialism unsuited to America. Actual circumstances and events forced them to contradict their stated principles and the principles on which imperial political institutions were grounded. And since this was a complex

³ See Hugh M. Hamill, Jr., "Royalist Counterinsurgency in the Mexican War of Independence: The Lessons of 1811," *Hispanic American Historical Review*, 53 (1973): 470-89. I would take his statement that excessive concentration on the insurgents "tends to warp our understanding of what actually happened" and apply it equally to excessive concentration on Spanish government and imperial administrative reforms in the century before the outbreak of insurrection, on the grounds that such concentration tends to make us think we have an explanation for American independence when, in fact, that remains to be demonstrated by specific application.

time not suited to analysis by use of clear-cut extremes, the reader will be presented a second—and long overdue—thesis: that the viceroys were not incompetent, but on the contrary, extraordinarily competent politicians and military leaders. A myriad of forthright actions and forceful decisions taken by the viceroys to solve actual problems facing them ultimately proved to uncommitted Americans that Spanish imperialism was no longer valid, that it was false authority. Mere inaction could not have accomplished that, nor could mere rebel propaganda, nor could even the remarkably clearheaded political analyses of Bolívar himself. Independence was more than a coup d'état, though it was also something less than true revolution if that term be taken to require social, economic, and even intellectual revolution. It was the rejection of a three-hundred-year-old political tradition and of a previously held identity. Opposition alone could not have produced such a profound political change; established authority must first have proved itself invalid, and the viceroys were the principal agents of established authority.

The subjects of this article are the men who in their role as “alter ego,” literally “vice-king,” of King Ferdinand VII, represented in their persons the authority of the sovereign and the imperialism of Spain in the two chief American colonies. For New Spain they were Francisco Xavier Venegas, marqués de la Reunión de Nueva España (1810–13); Felix María Calleja del Rey, conde de Calderón (1813–16); and Juan Ruiz de Apodaca, conde del Venadito (1816–21). For Peru they were José Abascal, marqués de la Concordia (1806–16); Joaquín de la Pezuela, marqués de Viluma (1816–21); and José de la Serna, conde de los Andes (1821–24).⁴ In order to view them properly we must remember that emotionally inspired antipathies have long obscured the record of their remarkable accomplishments. Together they provided the strongest leadership the American kingdoms ever had with the exception of the great sixteenth-century founder-viceroys. In every sense of the word, they actually ruled America. Their strength of purpose and loyalty help explain why independence took so long to achieve and why it cannot be assumed to have been inevitable. By definition they were imperialists, by training absolutists, and as wartime leaders they were responsible, as were the rebel leaders, for the destruction of the wars. Having said this about them, we have merely reaffirmed that they were effective servants of their sovereign. It is no more valid to dismiss Calleja from study because he was viewed by his enemies as a bloody butcher than it would be to ignore Hidalgo because of the atrocities committed by his followers. It is no more valid to dismiss Abascal as a mere reactionary than it would be to forget that Bolívar was no democrat. And yet, traditionally in Latin American historiography they have been viewed as the blundering, bloody-minded, and unthinking agents of an outmoded despotism that, as the cliché about the royal dynasty they served would have it, never learned anything and never forgot anything.⁵ This is

⁴ Each will be referred to hereafter by family name since some possessed their title while viceroy and others only after leaving their posts.

⁵ The most significant mark of this is the startling dearth of biographies of the viceroys; they have hardly been endowed with humanity. One would think America was governed from 1810 to 1824 by automatons. A

surely a disservice to history, for to view them as stereotypes is, among other things, to deny the full impact of the movement for independence and to lessen the stature of the Liberators who met and defeated them in a contest whose outcome was by no means preordained. It is to ignore the political opinions of that significant number of Spanish Americans who did not want independence and to whom the viceroys were saviors—it still remains to be demonstrated whether they were a minority or a majority, for the victory of independence does not constitute automatic proof. Above all, the traditional view of the viceroys denies rationality to Spain's imperial ethos.

How did the viceroys conceive of themselves and of their role? Each, of course, was very different, and the frequent disagreements among them were one source of their ultimate failure. What united them was chiefly the fact that they faced the task of revivifying viceregal authority in the face of universal assault from both America and Spain. All served the unworthy Ferdinand VII, who never failed to reward them (each was granted his title of nobility on the grounds of service as viceroy) but whose weakness and vacillation seriously undercut their authority. The extent of their love for Ferdinand himself is impossible to discover and not important anyway, as it was rather to the king as a symbol that they were loyal. Although each was a professional servant of the king, and therefore predisposed toward the defense of the king's prerogatives, which were also the viceroy's, each was genuinely dedicated to what they all conceived of as the only possible and correct foundation for the state—the absolute monarchy as represented by Ferdinand's grandfather Charles III (though they disagreed on the extent to which reform within the structure was desirable). More than that, they were also dedicated to their definition of Spain and of its role in the world, to Hispanism. Their proclamations referring to the brotherhood of Spaniards and the unity of the empire were not empty rhetoric. They believed that the brilliant civilizing mission of Spain in America was still alive. The noble titles they were

summary bibliography on the viceroys themselves is difficult because much of the most useful information is treated indirectly. For an assessment of Venegas, see, for example, Hamill, "Royalist Counter-insurgency." For a treatment of the reactions to Calleja's policies, see N. M. Farriss, *Crown and Clergy in Colonial Mexico, 1759-1821* (Oxford, 1968). Lucas Alamán's *Semblanzas e ideario* (2d ed.; Mexico City, 1939) includes biographies of the Mexican viceroys. Less useful is Artemio de Valle-Arizpe's *Virreyes y virreinas de Nueva España* (2d ed.; Mexico City, 1947). Carlos María Bustamante's *Campañas del General D. Félix María Calleja del Rey* (Mexico City, 1828) is a standard. It is interesting that there is a biography of Calleja's wife, who was an American: see José de Núñez y Domínguez, *La virreina mexicana: Doña María Francisca de la Gándara de Calleja* (Mexico City, 1950). The latest statement on Calleja is Carol Ferguson, "The Spanish Tamerlane?: Félix María Calleja, Viceroy of New Spain, 1813-1816" (Ph.D. dissertation, Texas Christian University, 1973). For the Peruvian viceroys the best overall biographies are in Manuel de Mendiburu, *Diccionario histórico biográfico del Perú* (Lima, 1874-90), in which the viceroys are treated in short original monographs. The well-known memoirs of Abascal and Pezuela are essential: José Fernando de Abascal, *Memorias de gobierno*, ed. Vicente Rodríguez Casado and José Antonio Calderón Quijano (Seville, 1944), and Joaquín de la Pezuela, *Memoria del gobierno*, ed. Vicente Rodríguez Casado and Guillermo Lohmann Villena (Seville, 1947). See also Fernando Díaz Venteo, *Las campañas militares del Virrey Abascal* (Seville, 1948), and Jaime Eyzaguirre, *La logia lautarina y otros estudios sobre la independencia* (Buenos Aires, 1973). La Serna and Apodaca in particular have been ignored. Original material and documents relating to the viceroys, however, are widespread, with the exception of La Serna, whose papers are badly scattered. A particular problem is that by special dispensation none of the six viceroys had a *residencia*, nor did these viceroys leave instructions to their successors.

granted—Concordia, Reunión—often reflected this mission. They embodied Spain's imperial ethos—absolutism, the sovereign, and the mission to spread and preserve true Christianity and true civilization.

The reader will already have scoffed, perhaps, wondering on what grounds he shall take these viceroys seriously if this was genuinely their concept of their role in America. Surely they could see that it was a concept belonging to the sixteenth century; surely they could see the illogic in it. No, in fact, they did not, or only partially at any rate, for they still genuinely believed it. They were, after all, imperialists, whose world view included belief in the rationality of their nation's history and their own actions. They could not grasp the point of view that Bolívar personified, the idea that Spain's mission in America was *over and that the child had outgrown the parent. At any rate, to assume that in 1810, or even in 1820, most Americans recognized the invalidity of Spain's civilizing mission in America is to anticipate. Its validity was proven, as Spaniards viewed it, by three hundred years of history.*⁶ Inertia, time, upbringing, and tradition were all on the royalists' side. The rebels did not win unanimous agreement when they declared that Spain's hour had passed. It had to be proved, and only the chief agents of Spain could prove it.

That is exactly what they did, for the fundamental factor that would destroy them, a factor they did not and could not recognize, was that the values upon which their authority and the Crown's authority were based were rapidly becoming irrelevant. On a wider spectrum, of course, world events disproved the divinity of the monarchy, the Napoleonic wars showed that Spain was no longer Europe's foremost power, and North American independence suggested that America could function independently of its founders. But within the empire itself events were showing the viceroys to be, unknown to themselves, defenders of a glorious irrelevancy. The things they claimed and thought they represented no longer corresponded to what they actually represented in the minds of Americans. The reality was a king who had overthrown his own father (it was the first time this had happened in the history of the unified monarchy), a war-torn Spain either subjected to Napoleonic rule or divided between constitutionalism and absolutism, and for many Americans, especially the nonelite, a very real oppression. The Napoleonic conquest of Spain and the questioning of the nation's fundamental traditions exemplified by the Constitution of Cádiz together contradicted or disproved the political

⁶ For studies illustrating the extraordinary staying power of Hispanism after independence, see Mark J. Van Aken, *Pan-Hispanism: Its Origin and Development to 1866* (Berkeley, 1959), and, among the most recent, Fredrick B. Pike, *Hispanismo, 1898-1936: Spanish Conservatives and Liberals and Their Relations with Spanish America* (Notre Dame, 1971). The basic emotion, which Pike succinctly defines as "an unassailable faith in the existence of a transatlantic Hispanic family, community or *raza* (race)" (p. 1), is not, after all, very different from the fundamental principle of the British Commonwealth, but its political, social, and philosophical meanings are very different. Since Hispanism is conservative, antimodern, antidemocratic, sometimes ultrareligious, and sometimes anticapitalistic, North Americans often find it incomprehensible. This does not alter the fact that it is very real and has answered the needs of countless Spanish American philosophers since independence. In the period under consideration here it was just beginning to be challenged. Throughout the remainder of the nineteenth century, Hispanism in Spanish America was at its ebb, owing to the bitterness caused by the Wars of Independence, but it revived after 1898 when Spain was no longer a threat to American safety.

values Americans had previously been taught to believe. The viceroys sensed this crisis of confidence and responded to it in different ways, but they never fully understood it. It was a fault in the system, not in themselves, but they were the agents of the system and therefore of its failure.

JOSÉ ABASCAL, WHO GOVERNED the Peruvian viceroyalty for ten years, was the most absolutistic in his response to the rebellion. Perhaps he best understood the authority that the viceroy traditionally personified. He was convinced that standing firm, not moving an inch, was the best defense against the crises he faced on all sides. Indeed rebellion provoked in him greater adherence to absolutism. In a report to Spain in 1815 he accused even the Lima Inquisition of weakening his authority by daring to criticize him. As a witness to the revolution, he testified, "I know that nothing has so prejudiced the king's cause as the lack of resolution, or the imbecility, of those who have held power" in America. In 1814 he had a disagreement with his own *audiencia* (royal court), which he accused of being too easy on a rebel sent for trial from Arequipa. When the *audiencia* referred to their disagreement as a "conflict," the viceroy replied, "I urge you next time to avoid using the word 'conflict' with me, because either you do not understand its significance, or you forget where I come from and what I represent."⁷ To him a conflict between the two arms of royal authority in Peru was a contradiction in terms. This was the tone adopted by the viceroys who were in office when the insurrections broke out, for maintaining Spain's imperial power undiminished was the viceroys' appointed task.

Viceroy Felix María Calleja of Mexico was also a genuine absolutist who, unlike every other viceroy, spoke and wrote with a stunning frankness about the royal government's troubles. He came closest to understanding what was actually happening. Following the nullification of the Constitution upon Ferdinand's restoration in 1814, Calleja wrote a remarkable letter in which he explained to the peninsula that "the ancient illusions" of the Americans about the authority of the Crown and its agents had received a death blow from the liberalization and confusion of authority that the Constitution entailed. With remarkable political perceptiveness he pointed out that what was important was not so much the defeat of one or another rebel chieftain but the restoration of what he frankly recognized to be the great myths that had cemented the state. The restoration of calm sufficient to allow a return to normality was also vital, "for even if the arms of the rebels prove unsuccessful . . . still misery, and a growing consumption, will do that which neither force nor intrigue may be able to effect."⁸

Abascal and Calleja, then, had the clearest and coldest understanding of

⁷ Abascal to secretary of the Indies, Lima, Mar. 29, 1815; Abascal to minister of grace and justice, Lima, Aug. 2, 1814, both in Archivo General de Indias, Seville (hereafter AGI), Lima 749, 748.

⁸ Calleja to minister of grace and justice, Mexico City, Aug. 18, 1814, AGI, Mexico 1482; the translation is from Henry George Ward, *Mexico in 1827* (London, 1828), 1: 512-22.

authority and its employment, which is power. Almost as a direct adjunct, they were the most successful of the viceroys in fighting the insurrection. In Peru, Abascal was able virtually to prevent the insurrection from spreading into the viceroyalty itself, while he raised money and dispatched troops to



Fig. 1. José Abascal, viceroy of Peru, 1806–16. The six portraits appearing throughout the article are late nineteenth-century drawings taken from the official portraits made of each viceroy during his reign. Each wears the uniform of captain general and personal decorations.

help restore royal governments in Montevideo, Upper Peru, Chile, and Quito. Calleja, the military genius who took office at the high point of rebel fortunes in Mexico, was able in three years to break the rebellion's back, to capture Morelos and destroy his forces, and to pacify almost all of the country, so that the year after Calleja left office his successor, Apodaca, could claim that the rebellion in Mexico was over.⁹ All this was accomplished even though, as Calleja wrote, "The war strengthens and propagates the desire for Independence, holding out a constant hope of our destruction, a longing desire which . . . is general amongst all classes, and has penetrated into every corner of the kingdom." He recognized the hollowness of his military victories and insisted that the only salvation from destruction was "to reanimate the authority of the government."¹⁰

⁹Apodaca to minister of war, Mexico City, Oct. 31, 1816, Archivo General de la Nación, Mexico (hereafter AGN), Historia, vol. 152, no. 2.

¹⁰Calleja to minister of grace and justice, Mexico City, Aug. 18, 1814.

Slightly less cynical, but still undeniably absolutist, was Francisco Xavier Venegas, Calleja's predecessor in Mexico. It was he who had to face the shock of Hidalgo's uprising in 1810, the bloody first round of the Mexican war. Within days after his reception in the capital in September 1810, there appeared a pasquinade on the walls of the viceregal palace mocking his personal appearance and style of dress. In typically direct fashion, he is said to have ordered an answering pasquinade to be affixed next to the offending original with the words:

My face is not that of an Excellency
nor my clothes of a Viceroy,
but I represent the King.
This simple advice
I give you for what it is worth:
The law must be the north star
that guides my actions.
Look out for treacheries
done in this court.¹¹

It was Venegas's sagacity, especially his ability to choose extremely competent officers like Calleja, who was commander of the army of the center and later military governor of the capital, that allowed him to resist the terrifying uprising of the Indian masses under Hidalgo and the guerrilla war under Morelos that followed. In 1811 he faced two direct plots by dissidents in the capital to kidnap or assassinate him. He organized new militia groups, firmly resisted the more radical requirements of the Constitution after 1812, opened new sources of revenue in the face of genuine financial crisis, and ended the dangerous lack of direction the government had suffered from since the shocks of 1808. In 1810 and 1811 he created a special police force for the capital city and special tribunals to deal with treason and rebellion throughout the nation. He denied charges by the city council of Mexico that these agencies were indistinguishable from those of the French tyrant in Spain,¹² and he ignored orders from the Cortes to disband them.

The greatest danger, however, to the authority of all three of these viceroys came from Spain, not from the American insurgents. In 1812 the erosion of the fundamental principles of the empire reached its peak in the publication of the Constitution written by the Spanish Cortes of Cádiz. The Constitution, which the viceroys had to declare in America if only because it was the work of the single commonly accepted legitimate government, lowered the viceroys to the status of "superior political chiefs" of their kingdoms, created elected provincial deputations to share power with viceroys, reduced *audiencias* to mere courts of law, and established elected city governments. Most startling, it declared national sovereignty vested in the Cortes rather than the absent king, a direct

¹¹ Quoted in Jesús Romero Flores, *México: Historia de una gran ciudad* (Mexico City, 1953), 481.

¹² Venegas to Mexico City council. Mexico City, Oct. 29, 1811. Archivo del Ex-Ayuntamiento, Mexico City, Policía en general, vol. 3629, exp. 176.

contradiction of the fundamental principle of the Spanish state. For two years the Constitution remained in effect until the restoration of Ferdinand nullified it, and for two years Viceroy Abascal in Peru and Viceroys Venegas and Calleja in Mexico agonized over the delicate task of appearing to execute its pro-



Fig. 2. Francisco Javier Venegas, viceroy of New Spain, 1810-13.

visions while ignoring those they perceived to be destructive of their authority. They were in the altogether extraordinary position—and one which the true absolutist would not have expected to encounter—of serving a metropolitan government that spoke for the king but that was controlled by a philosophy inimical, as they viewed it, to the true interests of the king. In Peru, Abascal nullified the Constitution's provision for the freedom of the press, paid only nominal attention to the provincial deputations, and struggled to neutralize the revolutionary effects of a freely elected Lima city council, which he thought represented Creole dissidents. In Mexico, Venegas faced the same problems, nullifying elections that came too close to threatening royal prerogatives and first implementing, then nullifying, the freedom of the press. Venegas and Calleja publicly quarreled over what Calleja viewed as the viceroy's insufficiently hostile attitude toward the Constitution and his unwillingness to prosecute the war militarily. Mexican reactionaries bombarded

Spain with requests for Venegas's replacement by Calleja, and in March 1813 Calleja took office. Promising to implement the Constitution fully in his first proclamation to the people as viceroy, Calleja nonetheless took no action to implement the free press, even in the face of fierce complaints from every level of the moderate faction in the country. Charges and countercharges were dispatched to Spain in bewildering numbers. All three viceroys had occasion to accuse dissidents of engineering local elections, while Creoles and liberals in both countries charged them with tyranny and illegal acts.¹³

Viceroy Abascal in Peru could afford to be less heavy handed in his efforts to neutralize the Constitution, largely because the kingdom was not itself a theater of war except in 1814, following the uprising at Cuzco.¹⁴ He attempted instead to direct the actions of the various constitutional agencies by actually giving the appearance of participation. The Constitution made the viceroy titular head or president of the city council of his capital and of the provincial deputation of the capital-province, and Abascal actually filled those chairs, something his Mexican colleagues refused to do. In this way he could supervise the actions of the councils. In the provincial deputation, for example, he personally appointed the secretary, while a year later he intervened in the electoral junta that was choosing a Cortes delegate from Lima.¹⁵ In 1813 he disqualified the elector chosen by Lima from participating in the vote to elect the Cortes delegate and provincial deputation on the grounds that the man chosen was a magistrate, and yet he was actually thought to be too well disposed toward the dissidents. In 1813 he disqualified one of the men chosen as a city councilor in Lima.¹⁶ He ordered the Lima city council to inform him whenever it expected to discuss a matter of major importance so that he could preside over the debate, and he demanded that it not write directly to the government in Spain without his approval, although it firmly refused to obey.¹⁷ He even refused to let the newly elected Lima city council for 1814 take the traditional *paseo* through the streets on the day of its inauguration.¹⁸ Abascal's real object was to allow the liberal provisions of the Constitution to draw dissidents out in the open so they could be identified. As early as mid-1813, meetings of his junta of war were able to discuss with remarkable accuracy the status of prominent citizens throughout the country

¹³Material on the Constitution and its effects is extensive, although it concentrates on Mexico. The most useful recent material includes, for Peru, James Larry Odom, "Viceroy Abascal versus the Cortes of Cádiz" (Ph.D. dissertation, University of Georgia, 1968); and, for Mexico, Nettie Lee Benson, ed., *Mexico and the Spanish Cortes, 1810-1822: Eight Essays* (Austin, 1966); Benson, *La diputación provincial y el federalismo mexicano* (Mexico City, 1955); Benson, "The Contested Mexican Election of 1812," *Hispanic American Historical Review*, 26 (1946): 336-50; and James F. King, "The Colored Castes and the American Representation in the Cortes of Cádiz," *Hispanic American Historical Review*, 33 (1953): 33-64.

¹⁴As J. R. Fisher has shown, however, the Constitution itself and Abascal's refusal to implement it fully were central causes of the Cuzco uprising, which we name after its Indian leader, Pumacahua. *Government and Society in Colonial Peru: The Intendant System, 1764-1814* (London, 1970), 227.

¹⁵Pedro Alcantara Bruno to regency, Lima, Mar. 13, 1814; Miguel Tenorio and others to Cortes, Lima, May 24, 1814, AGI, Lima 799.

¹⁶Miguel de Eyzaguirre to regency, Lima, Apr. 3, 1813, AGI, Lima 799; Actas del Cabildo, Lima, bk. 43, Mar. 23, 1813, Biblioteca Municipal de Lima.

¹⁷Abascal to secretary of Ultramar, Lima, May 31, 1813, AGI, Lima 745.

¹⁸Actas del Cabildo, Lima, bk. 43, Jan. 1, 1814, Biblioteca Municipal de Lima.

who in the years ahead would remain leaders of the underground in favor of independence.¹⁹

Venegas and Calleja were more direct in their opposition to the Constitution and consequently caused far greater public reaction, but they



Fig. 3. Felix María Calleja del Rey, viceroy of New Spain, 1813–16.

viewed their opposition as necessary, for the years of the Constitution corresponded with the high point of Morelos's campaign. Venegas actually annulled the first elections that took place in Mexico City on November 29, 1812, claiming they had been improperly conducted.²⁰ The night of the elections there were widespread popular demonstrations of support for the Constitution in the capital, which both Venegas and General Calleja called riots in their reports to Spain.²¹ After allowing the free press provision to go into effect for two months, Venegas annulled it as well, on the grounds that it gave cover to rebel propaganda. The fact that in a series of elections in the capital city hardly any peninsular Spaniards were ever elected conclusively showed the direction of popular feeling, and it explains Venegas's hostility.

¹⁹ Abascal to secretary of the Indies, Lima, Mar. 27, 1815, AGI, Lima 749.

²⁰ Benson, "Contested Mexican Election of 1812," 336–50.

²¹ Venegas to minister of state, Mexico City, Dec. 27, 1812; Calleja to minister of grace and justice, Mexico City, June 16, 1813, AGI, Mexico 1322.

On March 4, 1813, in the midst of the constitutional era, Calleja became viceroy. Speaking words of sweet reasonableness, he permitted the long-overdue elections of Mexico City's council and provincial deputation, but he delegated the reactionary intendant-corregidor of the province to serve as president of both. Promising everything, he simply neglected to do anything about restoring the free press, ignoring several direct orders from the Cortes to do so. He, too, was able to draw out dissidents by pretending to permit constitutional provisions. The elections were perfectly suited to that purpose. At one point he even negotiated with the famous underground rebel group called the Guadalupe as a means of discovering the loyalties of prominent residents of the capital. The rebels of the Guadalupe wrote to Morelos the day after Calleja's assumption of office and paid him the supreme compliment of warning that the viceroy was their greatest opponent, for, they said, "he is a great politician."²²

Calleja now commenced a military, political, and propaganda campaign against the insurgents that clearly defines him as the fiercest, most competent, most ruthless, and, from the royalists' point of view, best viceroy of the era. He publicly promised "to dedicate myself exclusively to the destruction of Morelos."²³ In 1814 he swore to the king that he would not let Mexico go while he remained in power even if he had to march at the head of the whole army across the country, laying it waste with fire and sword until every rebel was destroyed.²⁴ Special courts-martial of the most dubious legality were set up in the provinces to deal with treason, and they were ordered to ignore the immunity of clerics from civil prosecution and to execute rebel priests without ado. By June 1814, even before he had heard of the king's nullification of the Constitution, Calleja was sufficiently powerful to exult in a public decree, "Nothing can now stand in the way of the execution of my ideas."²⁵ He reacted "with unspeakable joy," as he wrote himself, upon hearing of the king's restoration.²⁶ With icy hauteur he commanded the dissolution of the various constitutional bodies as order after order arrived from Spain. When the constitutional city council of Mexico wrote him what he considered to be an insufficiently warm letter of thanksgiving following announcement of the restoration, he ordered it to write him again within four hours making it clear "whether or not you are disposed to guard, obey, and execute on your part everything touched on by His Majesty in his decree . . . annulling the Cortes and the Constitution."²⁷ Several months later the king wrote Calleja approving of all his previous actions, including his refusal to obey the

²² Los Guadalupe to Morelos, Mexico City, Mar. 5, 1813, AGI, Mexico 1482. On the Guadalupe, see Wilbert H. Timmons, "Los Guadalupe: A Secret Society in the Mexican Revolution for Independence," *Hispanic American Historical Review*, 30 (1950): 453-99, and Timmons, *Morelos: Priest, Soldier, Statesman of Mexico* (El Paso, 1963).

²³ Proclamation of June 22, 1814, quoted in Bustamante, *Campañas del General Calleja*, supp., p. 10.

²⁴ Calleja to minister of grace and justice, Mexico City, Aug. 18, 1814.

²⁵ Proclamation of June 22, 1814.

²⁶ Calleja to minister of grace and justice, Mexico City, Aug. 18, 1814.

²⁷ Calleja to Mexico City council, Aug. 22, 1814, Archivo del Ex-Ayuntamiento, Historia en general, vol. 2254.

Constitution, and authorizing him to take whatever measures necessary to stop the insurrection.²⁸ Having now a fairly complete list of secret traitors among the upper classes of Mexico City, throughout 1815 Calleja ordered a series of arrests of prominent nobles and gentlemen, city councilors, lawyers, and priests, which virtually destroyed the rebel fifth column in the capital. Meanwhile Morelos was captured and executed. Calleja was triumphant; his admirers called him the “Reconqueror,” the “Second Cortés.”

THE CONSTITUTION WAS GONE. But both Abascal and Calleja knew that its effects were far more widespread than their contemporaries, or even their successors Pezuela, La Serna, and Apodaca, suspected. The damage lay not in any temporary advantage the Creoles or dissidents had achieved, not in the confusion and chaos that had reigned, not in the fury of liberals and moderates alike who had seen their opportunity for a government of laws trampled underfoot by self-willed men. The chief damage lay in what the Constitution had done to the foundations upon which viceregal and royal authority rested. The Mexican *audiencia* called it a loss of Spain’s “moral force.”²⁹ In a letter dated August 18, 1814, Calleja affirmed that the Constitution had removed from the viceroy every vestige of authority he had possessed outside the use of plain force, and it could never be recovered. “The insurrection is now so deeply impressed and rooted in the heart of every American,” he wrote, “that nothing but the most energetic measures, supported by an imposing force, can ever eradicate it.” The Constitution had exposed the ministers and magistrates to ridicule. “They have lost their *prestige*, and even their respectability.” It was now too late; the mere defeat of the insurgents, he said, would not end the rebellion. This was so because continued warfare “acts against us in two ways: by open force, and by increasing distress; the first will always be repelled, the second will reduce us gradually to death’s door.”³⁰ Calleja, the Second Cortés, the most astute and ruthless of the last viceroys, recognized that he was trapped, for the only way to retain power was to use force, and force, he knew, was counterproductive. The more battles Spain won the fewer supporters it had, the more power the viceroys amassed the less true authority remained to the agents of Spain.

This replacement of authority by force was manifest in a remarkable exchange of letters between Calleja and the liberal bishop of Puebla, Dr. Antonio Joaquín Pérez. The bishop wrote the viceroy in April 1816, complaining about the cruelty of royal troops and the destructiveness of the war in general, and Calleja replied justifying his government’s and army’s actions. Calleja did not deny the excesses of the royal troops but justified them by citing the excesses of the rebels. Pérez said farms and factories of those

²⁸ See Francisco de Paula de Arrangoiz y Berzábal, *Méjico desde 1808 hasta 1867* (Madrid, 1871), 1: 271.

²⁹ Mexican *audiencia* to Cortes, Mexico City, Nov. 18, 1813, quoted in Ward, *Mexico in 1827*, 1: 497–509.

³⁰ Calleja to minister of grace and justice, Mexico City, Aug. 18, 1814. For a modern restatement of the theoretical principle that Calleja clearly understood, see Ted Robert Gurr, *Why Men Rebel* (Princeton, 1970), 232.

suspected of treason were destroyed needlessly; Calleja said the government had been too soft. Pérez accused royal troops of demanding excessive supplies; Calleja replied it was the duty of the countryside to supply a marching army. Pérez complained that the army was guilty of capricious and unwarranted bloodletting when capturing rebel towns; Calleja replied that he could not restrain successful and victorious troops, the laws of war permitted every excess. Pérez alleged the royal government had published false accounts of battles; Calleja said falsifying news from the battlefields was justified on the grounds of political expediency.³¹ In all the history of colonial Spanish America there is hardly another instance, outside of the initial conquest phase, of so unashamed a dependence on naked force. Calleja knew that any other defense of his actions would be hypocritical. Indeed, to him even compromise was hypocritical.³²

In Peru, meanwhile, the aging Abascal also sensed the turning point in the war had arrived. While the world witnessed royal armies sweep triumphantly across the entire continent, so that by 1816 all of South America except the Río de la Plata was again reduced to royal control, he sensed that sheer force was the only thing left, for authority had evaporated. Whether he recognized it clearly or not, it was only a matter of time before simple force could no longer sustain the regime. The viceroy begged Spain to let him retire, while Spanish ministries were flooded with complaints against him and his arbitrary use of power.³³ Most of the Peruvian complainants were unaware that it was only Abascal and his arbitrary use of power that had stanching the flood of rebellion.

Simultaneously, however, the practical problems of the royal government were becoming acute. Contrary to its silver-inspired popular image, Peru had always been poor, and its cost of living had for centuries been one of the highest in the world—the result, of course, of an excess of silver and a shortage of everything else. Now bread was selling for fifty cents a loaf and would rise to a dollar by 1821, while the government staggered under an unbearable cumulative deficit of 12,000,000 pesos and a yearly deficit by 1814 of 1,500,000 pesos, and 150,000 artisans were out of work.³⁴ Spanish economic precepts were rapidly being disproved as well. No exertion, no force, no intransigence on the viceroy's part could rescue Peru from this inexorable plunge toward bankruptcy as long as rebellion existed anywhere on the continent. But Peru could not defeat the insurrection everywhere. Its resources were becoming overstrained, and its population could not bear such exertions much longer.

³¹ "Cuaderno de contestaciones entre el Virey de Nueva España y el Obispo de Puebla," copied July 12, 1816, AGI, Estado 31.

³² Gurr explains: "Regimes facing armed rebellion usually regard compromise as evidence of weakness and devote additional resources to military retaliation." *Why Men Rebel*, 232.

³³ Domingo Sánchez Revata to Infante D. Carlos María, Lima, July 29, 1816; Mariano Trammarria to king, Lima, May 1, 1816; Antonio Arroniz to king, Lima, June 28, 1815; minutes of council of the Indies, Feb.–Nov. 1816, AGI, Lima 773, 1017. The most extreme charge was that Abascal was prolonging the war for his own glory and personal benefit.

³⁴ Report of Abascal to Junta de Arbitrios, Lima, Apr. 28, 1815; Lázaro de Ribera to Pedro de Macanaz, Lima, Feb. 3, 1815, AGI, Lima 741, 773. A Peruvian silver peso was equal to the United States dollar in 1810.

And all these financial difficulties existed prior to the final rush to independence of Peru's neighbors.

IS IT REALLY ANY WONDER, then, that Bolívar, writing from exile in Jamaica in 1815 at the nadir of his career, could declare that although the rebellion had been crushed almost everywhere in America, "success will crown our efforts, because the destiny of America has been irrevocably decided; the tie that bound her to Spain has been severed. Only a concept maintained that tie and kept the parts of that immense monarchy together. That which formerly bound them now divides them." The tie that bound consisted, in his own words, of "the habit of obedience; a community of interest, of understanding, of religion; mutual goodwill; a tender regard for the birthplace and good name of our forefathers." The concept that maintained that tie was Spanish authority. All that had disappeared, and Spain was now reduced to "an aged serpent, bent only on satisfying its venomous rage."³⁵

This well-known quotation of the Liberator, read and remembered by later generations with the most intense affection and pride, was certainly part prophecy. But it was also a foresight of what was soon to be manifest. The Liberator was predicting that the back of the royalist cause was broken, and at the very moment that royal power seemed most ascendant. How can this apparent contradiction be explained? Simply by remembering that the possession of territory by armies, especially in a civil war, is not the same thing as loyalty. In fact there were two wars going on: one a struggle for territory, the second a struggle for men's minds. While the first was important, the second was decisive. The Spaniards were winning the first but losing the second. Bolívar knew, if only intuitively, that the authoritarianism of both Abascal and Calleja and their cynical refusal to conform to the empire's fundamental law code had broken both the chain of affection upon which loyalty depended and the habit of obedience that was the foundation of royal power. The very ground rules by which loyalty was conceived had been altered by the viceroys' responses to insurrection, converting the delicate strands of loyalty into hatred. This is more than saying simply that royal military operations provoked hatred. That those operations were conducted by men who in the name of loyalty refused to obey the fundamental law as set forth in the Constitution was the fact that was so critical. This converted absolutism into true tyranny. Spanish political philosophy had always recognized a difference between absolutism and firm government, on the one hand, and tyranny on the other. Ferdinand VII himself, in his decree of May 4, 1814, annulling the Constitution, could declare that he and his predecessors had never been tyrants. By definition the king could not be a tyrant because by definition he reflected and was the ultimate culmination of the wishes and aspirations of his people. But in America that definition was now collapsing, for this

³⁵ Simón Bolívar, "Letter from Jamaica," in *Selected Writings of Bolívar*, comp. Vicente Lecuna, ed. Harold A. Bierck, Jr., tr. Lewis Bertrand (New York, 1951), 1: 103-22.

government was not working, which made it “bad government,” and it did not adhere to the law, which made it tyranny. Even the most apathetic Spanish American was bound to notice this contradiction between theory and practice because Spain and Spanish America had always worshiped the law.

Why were the royal authorities unable to build a new affection, a new or revived habit of obedience, on the basis of their military victories? The chief reason is that once Calleja and Abascal had made unalterable and immovable authoritarianism the foundation of royal power, authoritarianism had to be maintained. Any attempt by their successors to diversify the foundations of loyalty stood little chance of success. Once the iron fist was uncovered, any attempt to glove it would appear to be either hypocrisy or weakness; both would encourage further resistance. Authority, once corroded, can never be re-established by force. It might, however, be maintained for an indefinite period of time by force,³⁶ but that force must be constant and unremitting, and such force would require resources Spain no longer had at its disposal. Over and over again the next viceroys appealed to Spain for troops and warships. One need only read the diary of Pezuela in Peru, as he daily assessed the chances of this or that expedition being gathered and making it to Lima, to witness an unparalleled exercise in frustration. And besides, peninsular Spanish intransigence toward any reform in the years 1816–21 paralyzed the several viceregal initiatives toward compromise that were attempted.³⁷ American reform was still possible in this period if only because Spain once again controlled most of the territory of America. But if the liberals who wrote the Constitution and governed Spain amid the chaos of 1808–14 were unwilling to accept the reforms in trade, taxation, and government proposed by their supposedly equal American colleagues in the Cortes, the councilors of the Indies who replaced them in 1814 were certainly even less well disposed. After 1820, when reform was once again feasible on Spain's part, it was too late on America's. Independence became the logical answer because either Spain refused to consider reform or her agents in America made a mockery of it.³⁸

Both Abascal and Calleja retired from their American viceroyalties in 1816, officially hailed as saviors, positively adored by conservatives, but despised by radical and moderate Americans alike. Calleja privately urged Spain to maintain the terror in Mexico, for it was the only means of completing the destruction of the rebels,³⁹ and in letter after letter written in retirement Abascal urged maintenance of every aspect of the absolutism in Peru. But the new viceroys never received this advice directly from their predecessors (neither Abascal nor Calleja left the usual detailed instructions to their successors that

³⁶ See Carl J. Friedrich, *Tradition and Authority* (New York, 1972), 121. The definition of authority used throughout is Friedrich's.

³⁷ For a résumé of the way in which peninsular intransigence counteracted Morillo's attempts in Venezuela either to reform the regime or to crush the rebels utterly, see Stephen K. Stoen, *Pablo Morillo and Venezuela, 1815–1820* (Columbus, 1974).

³⁸ Gurr explains that “inflexible, repressive responses intensify the hostility of dissidents and reduce their hopes of obtaining reform except through revolutionary transformation.” *Why Men Rebel*, 236.

³⁹ Calleja to marqués de Campo Sagrado, Mexico City, Sept. 6, 1816, AGI, Mexico 1322.

earlier viceroys wrote), and besides, as witnesses to the post-1808 Spain, neither of the new viceroys would have been inclined to take it. There was a certain “time-lag” factor at work here in regard to successive viceroys’ attitudes toward major political questions. One of the fundamental paradoxes of the Wars of Independence is that Abascal and Calleja, who governed during the first constitutional era, had left Spain to take up foreign assignments in the period before the traditions of absolutism had begun to collapse. Reform was to them inconceivable. But the viceroys who governed in the time of the restored post-1814 absolutism—Pezuela and Apodaca—had experienced the formative years of their careers during the Napoleonic struggle when Spain changed at such a dizzying rate that reform became a way of life, indeed a necessity for defeat of the usurper. They were not, unlike their predecessors, terrified of reform, even if they did not actively seek it.⁴⁰ Both “generations” were thus somewhat out of step with the politics they were required to enforce.

IN NEW SPAIN, Juan Ruiz de Apodaca, a naval commander, former ambassador to London, and former captain general of Cuba, pursued a policy of limited military activity and widespread granting of amnesties to former rebels. He hoped to rebuild Mexico by making use of the military gains of his predecessor. Even in northern Europe it was noted that Apodaca’s policy was one of trying to regain American affection rather than one of government through fear.⁴¹ On several occasions Apodaca criticized Calleja for his “fire-and-sword” policy and his extraordinary special war taxes. Between 1816 and 1820 he repealed four fundamental taxes Calleja had created to meet the cost of the war—a property tax, a forced contribution based on incomes, a forced lottery (all three had been new and nearly revolutionary when first introduced), and a group of taxes on horses and carriages. Based on the assumption that Calleja had broken the back of rebel resistance, Apodaca’s program was a conscious attempt to ingratiate himself to the Mexicans. But by sheer contrast it helped to weaken the public image of the regime, for, while Calleja had ended the military aspect of the insurrection, he had not quashed the desire for independence. It was a time of high intensity and great drama—no lull at all, as existing historiography would have it—for it would show whether a policy of reason could solidify the gains won by relentless force, it would show whether Spain still had a right to govern.

In Peru, Joaquín de la Pezuela, former commander of the army in Upper Peru, became viceroy. He, too, made only limited use of the militia and army his predecessor had built up, even though he was very close to the troops and

⁴⁰ For example, both Pezuela and Apodaca were willing to give the restored Constitution a fair trial, and both, particularly Pezuela, were willing, however grudgingly, to enter negotiations with the rebels. Pezuela, as we will see, actively sought commercial reform. La Serna also gave the Constitution a trial run, but because of the circumstances it was never fully implemented in the Sierra. He entered negotiations with the rebels when forced to by either rebel action or royal command.

⁴¹ Manuel Palacio Fajardo, *Outline of the Revolution in Spanish America* (London, 1817), 343.

staunchly defended their rights in every way, as for instance in his policy of giving them preference in civil employment.⁴² He, too, criticized his predecessor publicly and privately, in this case for Abascal's authoritarianism and his refusal to permit foreign traders to land on Peruvian shores. He, too, was more flexible, more concerned with establishing popular support than with maintaining the iron fist. As in Mexico, it was too late. Confidence could not be restored, and flexibility appeared to be weakness, encouraging dedicated rebels to hold out for another day, while the extreme Right chafed at what appeared to be viceregal inactivity. Both Pezuela and Apodaca got caught in this trap, and with identical results.

When the historian focuses on the chief agents of Spain rather than on the leading rebels or on the rebellion itself, one factor not previously noted about the Wars of Independence begins to suggest itself. It may well be that 1816 was the true turning point in the movement, the point at which Spain's power had faltered beyond the ability of its agents to restore it. This was chiefly because those agents, and indeed most of the rebels, did not recognize that the loyalties of Americans could not necessarily be measured by which army controlled which territories. In 1816 royal armies were everywhere victorious. It may have been, therefore, a lull in the rebels' fortunes, but it was not a lull in the story of the deterioration of Spain's power, because those victories had come at an excessive price. To reconquer America had required destroying its haciendas, communications, factories, and even some of its cities. It led, on the one hand, to such privation that, as Calleja had predicted, the imperial system's ability to feed and house Americans was destroyed, and on the other, to a loss of confidence that the mere presentation by the rebels of an alternative to royal government could never have accomplished. As Bolívar said, Spain was reduced to an object of hatred, and its very victories accomplished that. The viceroys who took power in 1816 did not understand this, just as their predecessors had not realized that they themselves were doing it. To oversimplify, the equation would be this: in the years 1810–16, when America might have been saved for Spain by compromise and flexibility, it was governed by force and absolutism contrary to the fundamental laws; in the years 1816–21, when force and absolutism had become the only source of strength, America was governed by men who sought alternatives.

It may be protested that well over half the story has yet to be considered. What about the resurgence of the rebels, the victory of San Martín in Chile, the heroic gathering together of Bolívar's forces and the magnificent tale of their struggles, and the reappearance of rebellion in Mexico under the leadership of Iturbide? The reply is that, of course, that is the second and greater half of the story of how the rebels won, but it may perhaps be no more than the denouement, although with a few surprises, of how the Spanish lost. Precipitating agents were still required to begin the final process in each country, and they were not long in coming. In Peru it was economic confusion and disintegration, while in Mexico it was political confusion.

⁴² Pezuela to secretary of Hacienda, Lima, Nov. 30, 1818, AGI, Lima 761.

PEZUELA'S FINAL CRISIS was the result of the overextension of Peru's resources that was already acute when he took office. The countdown began with the final loss of Chile in 1818 at the battle of Maypu. Pezuela's major preoccupation thereafter was his attempt to open Peru to free trade with Europe, North America, and Asia, an attempt that lost him the support of Peruvian merchants, the soldiers, and the homeland. A remarkable letter from Manuel Vidaurre, former minister of the Cuzco *audiencia*, clarifies the economic catastrophe that was sweeping Peru and that made Pezuela's commercial reforms necessary. Writing in 1817, Vidaurre told the king, quite simply, that the excessive harshness of the royal commanders in Peru was driving the population to prefer death. Endless oppression led to unwillingness or inability to work the land, and so to hunger. In Cuzco province wheat was then selling for twenty-seven pesos the *fanega*, at La Paz it was forty pesos. Entire towns had died of hunger, he said. In Moquegua war taxes on its chief product, brandy, had quadrupled its price; in La Paz war taxes on its chief product, coca, had quadrupled its price; in Lima war taxes on bread and grains and real estate had the same effect; while the loss of the Chilean wheat supply after 1817 forced the capital to depend on its own poor and unpopulated countryside. "When a man has nothing," concluded Vidaurre, "then he becomes a rebel, because in order to survive no other recourse remains to him but a resort to arms."⁴³ In May 1818, Pezuela convoked a junta of prominent individuals to find new sources of revenue. He told them that they needed an additional 200,000 pesos immediately for urgent expenses and an additional 117,000 pesos every month to cover deficits.⁴⁴ This money had to come from foreign trade, since no domestic sources remained, and so sudden an increase in customs revenues and indirect taxes from trade could only be accomplished by throwing Lima open to every passing vessel no matter what its nationality.

That was the rub, for every time Pezuela asked Spain for permission to allow free trade, the Consulado, the chief monopoly of Lima merchants that controlled foreign trade, resisted, as did Spain itself. As early as 1817 Pezuela was making occasional requests to Spain to allow individual foreign ships to land at Lima. By 1819 he made a request for total freedom of trade, even proposing regulations by which it would be controlled, and in 1820 he repeated the request.⁴⁵ In opposition, the Consulado claimed that foreign trade, especially British, would destroy Peru's industry and economy, while a report drafted by the former viceroy Abascal in Spain concurred, reminding the king that the English never withdrew once they gained a foothold in foreign ports and that their presence was always "very dangerous."⁴⁶ On

⁴³ Manuel Vidaurre to king, Lima, Apr. 2, 1817, AGI, Indiferente 1568. Vidaurre had been held in great suspicion by Abascal, partly because he had a habit of speaking his mind.

⁴⁴ Pezuela to secretary of Hacienda, Lima, June 16, 1818, AGI, Lima 1550.

⁴⁵ Consulado to minister of Ultramar, Lima, May 3, 1817; memorandum of Hacienda to minister of war, Madrid, Dec. 25, 1819; Francisco Xavier de Olarria to government, Madrid, July 29, 1820, AGI, Indiferente 313, Lima 1550, 1022.

⁴⁶ Consulado to king, Lima, Feb. 13, 1819; Abascal to secretary of Hacienda, Madrid, June 29, 1819, AGI, Lima 1550, 1505.

another occasion Abascal had declared that free trade “would be tantamount to decreeing the separation of [America] from the mother country since, once direct trade with foreigners was established . . . Spain would matter little to them.”⁴⁷ The demand for free trade, indeed, had long been a chief objective of



Fig. 4. Joaquín de la Pezuela, viceroy of Peru, 1816–21.

the rebels. The Consulado promised to make up the treasury deficits out of its own funds, but failed to make good its promise. In 1820 the Crown promised to appoint a commission to study the matter, but meantime Pezuela allowed almost every foreign ship that presented itself to enter and discharge its cargo at Callao and even at the lesser ports.⁴⁸

Thus Peruvians were presented with the extraordinary picture of the representative of royal authority publicly proclaiming the necessity of rejecting the commercial exclusiveness that had been the fundamental principle of Spanish American economics and a major grievance of the Creoles, while a former viceroy and the chief merchants opposed it. We cannot resist the conclusion

⁴⁷ Abascal to secretary of war, Lima, May 23, 1812, quoted in Fisher, *Government and Society*, 154.

⁴⁸ Pezuela to secretary of Hacienda, Lima, Apr. 26, 1817; Pezuela to Hacienda, Lima, Dec. 16, 1817; Pezuela to Hacienda, Lima, Nov. 3, 1818; palace memorandum, Madrid, May 10, 1819. AGI, Lima 756, 758, 759, 1550. Pezuela talks about this question at great length in his *Memoria* (which is actually a *diario*) and makes clear his disappointment with the Consulado. *Memoria*, 295, *passim*.

that Pezuela was right, but the nation's greatest economic powers put their own interests first. The Old Regime could not survive when the very corporations upon which royal power depended perceived their interests to be opposed by the royal power. In the economic sphere, too, the royal system



Fig. 5. José de la Serna, viceroy of Peru, 1821–24.

was disproving itself. By July 1819 the royal troops in Lima were on half salary, and by mid-1820 Pezuela knew that San Martín and the Chilean government had an expedition of twenty-eight ships and four thousand men ready to embark for an assault against Peru's coasts.

The net effect of this economic crisis was that, even before the San Martín expedition landed in Peru, Pezuela could predict its success. He wrote the peninsula in 1818 that there was little confidence in the royal government left, especially among the lower classes and troops of the militia.⁴⁹ Between the rebel victory at Maypu and the arrival of San Martín's expedition in September 1820, Pezuela's government was thoroughly discredited, and through it, the royal regime as a whole. Agents from Peru later reported to the peninsula that "the personal opinion of everyone was that Peru was being lost, not through lack of means of defense, not through the superiority of the enemy, but through the wrong system and lack of skill of Joaquín de la

⁴⁹ Pezuela to secretary of state, Lima, Nov. 12, 1818, AGI, Estado 74, doc. 31.

Pezuela.”⁵⁰ The veteran royal army remained intact, however, and desperately wanted to confront the Chilean expedition after it landed at Pisco. In the face of what the army interpreted as Pezuela’s refusal to strike against the rebels, nineteen of the chief officers garrisoned near Lima forced him to resign in January 1821. The officers chose Field Marshal José de la Serna, general in chief of the armies, to become new viceroy.⁵¹

La Serna’s regime could do nothing but depend on military force. The incomparably subtle and complex question of a people rejecting their past heritage and choosing a new form of government was now reduced to the arbitrary question of which army would win at battle. If San Martín’s forces had been stronger in 1821, La Serna would have been defeated quickly. In July 1821 the viceroy and the royal army abandoned Lima and fell back upon the ancient source of Peru’s strength, the Andes. The weakness and confusion of the independent republic established at Lima guaranteed several more years of life to the royal power in the highlands.⁵² La Serna claimed on many occasions that his abandonment of Lima saved the rest of Peru and that the nation surely would have been lost if Pezuela had remained in command.⁵³ But an army on the march was not the same thing as a royal government, and despite the valor and skill of La Serna and his commanders, the royal forces were defeated in battle by Bolívar’s forces in 1824, completing the process of independence. The La Serna administration, then, should best be viewed as merely a “last ditch stand,” even though the combination of the rebels’ military weakness and La Serna’s inaccessibility in the high mountains permitted it to continue for four years. He might have been able to establish an enclave, but to retake the coast would have required the aid of massive reinforcements of peninsular troops and the re-establishment of Spanish naval control of the Pacific, both impossible, and would simply have constituted a military conquest anyhow.

IN MEXICO, VICEROY APODACA took office in 1816. He was the most administratively skilled of the last viceroys, the most personable, and the most genuinely popular in his capital city, though as usual residents of other parts of the nation never saw him. He frequently spoke of his wife and five children, one of whom was blind. As a former ambassador he was attuned to the importance of communication and sent monthly summary reports on the state of the kingdom to Spain. The index alone of his letters and reports in office runs to sixty volumes.⁵⁴ Undoubtedly his most striking characteristic was his optimism and his belief that the righteousness and truth of the royal cause

⁵⁰ Marqués de Valleumbroso and Antonio Seoane to conde de Casaflors, Rio de Janeiro, June 29, 1821, AGI, Indiferente 313; see also an anonymous letter, Lima, Apr. 30, 1821, AGI, Indiferente 1370.

⁵¹ La Serna to secretary of war, Lima, Feb. 9, 1821, AGI, Indiferente 313.

⁵² For a fuller description, see Timothy E. Anna, “Economic Causes of San Martín’s Failure in Lima,” *Hispanic American Historical Review*, 54 (1974): 657–81.

⁵³ La Serna to minister of grace and justice, Cuzco, Mar. 15, 1824, AGI, Lima 762.

⁵⁴ The manuscript indexes are in the Real Academia de la Historia, Madrid.

would prevail over the leaderless bandits who now made up the remnants of the radical revolutionaries. He never foresaw the possibility that his government would ultimately be destroyed not by the Left but by the Right. This was not necessarily naïveté, for his optimism was the natural result of his predecessor Calleja's very success at driving the rebellion underground, where not being apparent, it was more subtle, less easy to combat, and more likely to manifest itself later from a different quarter, since its earlier loci had been disrupted. When the Constitution was reinstated in 1820 Apodaca appeared to make a genuine effort to implement it. In a sense he had no choice in the matter, but unlike his predecessors he did not attempt, until it was too late, to intervene in and control the operations of the constitutional system. Elections were held regularly and without intervention, while freedom of the press was implemented for one full year, until after the Iturbide uprising began. Apodaca reported to the Cortes that he thought the reimposition of the Constitution had caused no unrest whatsoever in Mexico.⁵⁵

But it was the Constitution, in fact, that ultimately destroyed Spanish authority in Mexico, not by provoking a counterrevolution, as is sometimes alleged, but by proving to Mexicans the invalidity of the imperial ethos. The stunning royal decree of April 11, 1820, in which Ferdinand VII apologized to the American kingdoms for his error in annulling the Constitution in 1814 and declared that the ancient absolutism was wrong while begging dissidents to remember that "errors [in judgment] are not crimes,"⁵⁶ gave the final lie to the "tribal myths" that had permitted the Spanish system to function and around which Calleja had anchored his restoration of royal power. If the throne was not sacred, if it could give in so easily to internal revolt and overthrow the fundamental law to replace it with what in 1820 was Europe's most radical government, then nothing really stood in the way of an intellectual and philosophical acceptance of independence. This is exactly what was implied in a remarkable report to the peninsula from the councilor of the *audiencia*, José Hipólito Odoardo, in October 1820. Odoardo reported that the Cortes's radical legislation directed at restricting the power of the Church, the military, and the aristocracy throughout the empire had in only seven months completely redirected the loyalties of Mexicans, so that he could predict that some new uprising was imminent, though he could not foresee from which direction it would come, and that it would be successful in overthrowing the royal regime.⁵⁷ The viceroy himself received a similar prediction from the city councilor Francisco Manuel Sánchez de Tagle in January 1821.⁵⁸ The long expected overt threat to royal dominion finally came from Agustín Iturbide, a disaffected royal officer, who in early 1821 proclaimed an uprising that, because it represented a compromise of the

⁵⁵ Notice of receipt of report by Apodaca, Madrid, Oct. 17, 1820, AGN, Reales cédulas, vol. 224, no. 93.

⁵⁶ Proclamation of the king to the overseas inhabitants, Apr. 11, 1820, AGN, Impresos oficiales, vol. 43.

⁵⁷ Fiscal José Hipólito Odoardo to minister of grace and justice, Mexico, Oct. 24, 1820, quoted in Arrangoiz, *México desde 1808 hasta 1867*, 2: 12–16.

⁵⁸ Francisco Manuel Sánchez de Tagle, "Sobre que el sistema constitucional pierde cada día mucho de su valor y eficacia," AGN, Ayuntamientos, vol. 178.

wishes of both upper and middle class, immediately gained the support of the elite and amnestied rebels alike. In a most unusual twist of Spanish American history, the viceroy now represented reform while the insurgents represented a much more moderate program that threatened no one's status or wealth but



Fig. 6. Juan Ruiz de Apodaca, viceroy of New Spain, 1816–21.

simultaneously achieved national self-determination. Iturbide knew that a confused royal government that had contradicted its own principles could not survive. He told the viceroy, “Is there anyone who can undo the opinion of an entire kingdom? . . . Any country is free that wants to be free.”⁵⁹

Apodaca had never dreamt of such a threat, and in the face of it he had no defenses whatsoever. When he announced the uprising to Spain he admitted that “this unexpected event has filled the capital with as much surprise and consternation as it has me.” He warned that Iturbide was very dangerous because of his long association with chief officers and the Creole elite and because his program of independence—the Plan of Iguala—would inevitably “seduce” many of the wealthy, while it was equally attractive to genuine rebels.⁶⁰ It is almost incredible that Apodaca’s only reply to Iturbide’s treason was to offer him amnesty. By May 1821 Apodaca reported that a majority of

⁵⁹ Iturbide to Apodaca, Iguala, Feb. 24, 1821, AGI, Mexico 1680.

⁶⁰ Apodaca to minister of Ultramar, Mexico, Mar. 7, 1821, and May 29, 1821, AGI, Mexico 1680.

the troops and many officers had already deserted and that the kingdom was on the verge of being lost. As in Peru, royal authority had been defunct since 1816 and the viceroy's apparent paralysis and inability to get his orders obeyed led the minority of die-hard veteran army officers to view him as dispensable. Consequently, in an almost exact replay of the Peruvian incident, on July 5, 1821, Apodaca was forced to resign by his officers and was replaced by a field marshal, Francisco Novella.⁶¹ Iturbide's control of the nation was too far advanced, however, to permit Novella to retrench on some interior location as La Serna did in Peru, and independence triumphed only two months later. It was natural, for Iturbide genuinely embodied at that moment the wishes of the nation; he possessed, in other words, the genuine authority.

In a poignant letter describing these events after he fled to Cuba, Apodaca wrote, "I had a feeling of presentiment about this misfortune in the middle of last year, 1820, but not about the terms in which it would come about or the means by which it would be effected, because they are so extraordinary that it was not possible for anyone to imagine them."⁶² This was his confession that he had sensed the loss of authority that we have traced here but had not grasped its significance or understood it. There is no indication that any other royalist ever did either, except perhaps Abascal, who had recognized as early as 1808 that there was some indefinable core, some not quite explicable or demonstrable principle that was in danger of being lost if he gave the slightest hint of weakening his grip over all aspects of his country, and Calleja, who by 1816 knew it was already lost. That principle was Spanish authority. Once weakened, no amount of effort could have preserved the political and social institutions that were based on it.

THE INABILITY TO RECOGNIZE that Spain's authority in America had disintegrated continued to characterize the former royal magistrates for years afterward. They never quite accepted the fact that an event so unthinkable in 1810 had come to pass only a decade later. This lack of acceptance is part of the reason for Spain's unnecessarily long refusal to recognize the independence of its former colonies. Spain continued to imagine that some sort of restoration was possible. Significantly, two former viceroys, Venegas and Apodaca, were members of the Spanish council of state that in 1828 was still debating methods by which to "pacify" the "rebellious American provinces."⁶³ Indeed Spain as a whole never quite grasped what had happened until the unforeseen shocks of 1898 made its intellectuals and philosophers aware, with great pain be it remembered, that not only did they not possess a great empire, but they possessed no empire. For a nation to have the

⁶¹ Apodaca to secretary of Ultramar, Guanabacoa, Cuba, Nov. 17, 1821, AGI, Mexico 1680. For more detail on Novella, who should not be considered a viceroy because he had no royal confirmation, see Timothy E. Anna, "Francisco Novella and the Last Stand of the Royal Army in New Spain," *Hispanic American Historical Review*, 51 (1971): 92-111.

⁶² Apodaca to secretary of Ultramar, Guanabacoa, Cuba, Nov. 17, 1821.

⁶³ Council of state, Madrid, May 29, 1828, AGI, Indiferente 1564.

things in which it genuinely believes proved to be irrelevant, at least to the outsiders it thought it was convincing, is a terrible discovery. Before we scorn Spain for its *Quijotismo* in this regard (and this is also a part of the Black Legend about Spain) we should remember that every other imperialist power has suffered the same delusion. The very things that make imperialism possible—the combination of a once undeniably functional economic, governmental, and philosophical system with the missionary zeal and self-righteousness that derive from delusions of divine inspiration—guarantee that imperialists will never understand why subject peoples reject their political dominion.

The greatest loss involved in the disintegration of the Spanish authority in America, however, was that the independent states had no unanimously accepted authority to take its place. This was owing to the fact that when Spanish America found royal authority irrelevant—not just wrong or misguided but actually irrelevant, no longer suited to its conditions—then it also found the tradition upon which that authority was based irrelevant, at least insofar as it directly involved the political institutions. That part, at least, Spain had predicted. When the Cortes sent peace commissioners to South America in 1822 to negotiate truces and perhaps even settlements with the insurgents in Peru, Río de la Plata, and New Granada, the commissioners carried a set of secret instructions reminding them to tell Americans that independence would mean chaos, factionalism, political discord, and the loss of individual freedoms which the Cortes said its own Constitution would guarantee. The chief problem America now faced, the commissioners were informed, was an absence of authority, for authority had been replaced by “a thirst for power, which is what constitutes the overseas insurrection thus far.” In all the newly independent or almost independent states, Madrid promised, the lack of authority “has to produce terrifying evils.”⁶⁴ It was a fit last word of paternal advice, though no one listened.

Spain lost America because it lost its ability to prove its right to sovereignty, its ability to convince. In politics, economics, and religion it became irrelevant. The Crown, the king, his agents, and Spaniards themselves were no longer perceived to be necessary. The decadence of Charles IV and Ferdinand VII, the father’s forced abdication, the son’s detention in France, the Napoleonic conquest, the emergence of self-made government in the regency and Cortes, and the Constitution, all these did more to weaken Spanish authority, to make it—or prove it—false, than did all the rebels in America, for these events contradicted and disproved the values upon which the state was built. The forces arrayed against the Spanish Empire in America were mighty, but the most decisive of those forces were those that came from within, not from without.

At the center of this dilemma were the wartime viceroys, the men who bore the obligation of preserving Spanish power even as Spain changed so rapidly

⁶⁴“Previsiones muy reservadas que S. M. hace a los Comisionados,” Madrid, 1822, AGI, Indiferente 1570.

that the foundations upon which they attempted to anchor that power disappeared. In the only response they could conceive of to the undeniable threat posed by the outbreak of insurrection, Abascal in Peru and Venegas and especially Calleja in Mexico geared their governments, their armies, and their supporters to a do-or-die struggle between absolutism, untouched by any serious consideration of the possibility of reform, and rebellion. But this viceregal despotism occurred at the very moment that Spain claimed to be dedicated to reform. This authoritarianism converted the delicate strands of loyalty, faith in the monarch, and sense of brotherhood of all Spaniards into tyrannical government by foreigners over Americans. It enhanced the desire for independence. Then Apodaca in Mexico and Pezuela in Peru, not recognizing they were now the agents of tyranny, lost the military gains of their predecessors through confusion and a well-intentioned attempt to diversify the foundations of a system that had become dependent upon military force. Ultimately in Peru, La Serna's government that was no government held out in the mountains, protected until 1824 by the disorganization of its enemies. The viceroys could only operate on the understandings of politics and sovereignty that they possessed, of course. But since they were the living symbols of the dominion and majesty of Spain, they were also the living symbols of its confusion and, therefore, the agents by which it proved itself irrelevant to America.

American cries against the tyranny of Spain, which in 1810 were the product of a lack of restraint and propagandistic enthusiasm, became true and deeply felt by 1820 because the viceroys converted a system that was merely aged and in rare instances actually decrepit into genuine tyranny. What is perhaps worse, having done so they could not sustain it, proving to Americans the invalidity of their possession of power.

My contention, then, contradictory though it may appear, is that the war-time viceroys were one of the active forces that led Americans to reject their ancient imperial heritage, while at the same time they are the historiographical victims of that rejection. At the very least they were not the bloody-minded nonpersons of much of Spanish American historiography. At the most, they came close to preserving Spain's control of America. No one can read Calleja's candid letters to Spain and fail to recognize in him a political and military genius desperately struggling with a disintegrating situation. No one can read Pezuela's *diario* and fail to notice his genuine concern for Peru or the personal agonies he endured when he learned the news of Maypu, or heard of the loss of his son aboard the *María Isabel* captured by the Chilean navy, or of the death of his son-in-law Mariano Osorio in Cuba on his way home to Spain in a desperate attempt to explain the failure to reconquer Chile. We have too long concentrated on Abascal's mundane accomplishments—the Lima surgical college, the royal cemetery—at the expense of his greatest one—the preservation of Peru in the face of revolution from all sides. We have only recently begun to treat of Venegas in studies of Hidalgo. We have never credited Apodaca with the rationality and

administrative skill that his contemporaries greeted with sighs of relief. La Serna was the man who for four years fought so heroically that his conqueror Antonio José de Sucre treated him with genuine respect and felt real sadness at his personal humiliation at Ayacucho. Having recognized the viceroys to be well worth study, we can begin to become aware of the complexity of their impact on independence.

The Last Whig Historian and Consensus History: George Macaulay Trevelyan, 1876–1962

JOSEPH M. HERNON, JR.

“THE JUBILEE IS WELL, but one flies for the refuge of contrast to the French Revolution, to see that men can on occasion be discontent as well as content, that there is the everlasting Nay, as well as the everlasting Yea.” In tones of Carlyle the young George Macaulay Trevelyan, busily writing his dissertation for a fellowship at Trinity College, Cambridge, responded to the pomp and circumstance of Queen Victoria’s Diamond Jubilee in 1897. “The one event,” he wrote to his parents, Sir George Otto and Caroline Philips Trevelyan, “is the reaping of long assiduous sowing of the whirlwind, the present pageant is the reaping of wheat sowed on the whole in greater quantities than chaff. The Justice of God is manifest in both and the one is incomplete without the other.” The Jubilee “may superficially appear a triumph of toryism, but it is a result of the liberalism of the century. Imagine a progress of George IV through the streets of London. Such would the Queen’s progress be, if England had not got itself reformed somewhat in the last seventy years.”¹

The tastes and ideas informing this letter are among those which would dictate a good deal of Trevelyan’s historical writing, a body of work set in the “Whig interpretation,” as scholars call it, first popularized by his great-uncle, Thomas Babington Macaulay. A sometimes paternalistic humanitarianism, confidence in progress, a dedication to constitutional and parliamentary government, a trust in free educated intelligence and a belief that rational people can resolve or live with their differences of opinion, and above all a habit of moral self-discipline and submission to rational procedures of government: these Whiggish attitudes are so familiar to the British and American people as to seem less the components of a specific philosophy than the

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¹ G. M. Trevelyan (hereafter GMT) to his family [1897], Sir George Otto Trevelyan Papers, University of Newcastle-upon-Tyne, box 15. For references to the everlasting Nay and the everlasting Yea, see Carlyle, *Sartor Resartus*, in *Carlyle: An Anthology*, ed. GMT (New York, 1953), 57–61, 63–68.

given, implicit, and somewhat stuffy elements of our shared Atlantic culture. Even Whig historical work portraying English history as the story of their emergence and triumph sometimes treated these attitudes as self-evident goods to which rational and decent people would adhere as a matter of course. Yet the canons and disciplines of the Whig mind deserve to be recognized as constituting something more positive and distinct than that. Recent American radicals who have dismissed particular brands of academic and political Whiggery under the devastating term “liberal” have in effect given that recognition, treating liberalism as a specific philosophy and mode of conduct, fit to be rejected or taken up.

By 1911 G. M. Trevelyan had established an international reputation after publishing books on English medieval and Stuart history and the popularly acclaimed Garibaldi trilogy. At the same time, his father, Sir George Otto Trevelyan, was awarded the Order of Merit in tribute to his six-volume *American Revolution* (1899–1914), and G. M. Trevelyan, reflecting on the distinction in a letter from Venice, discussed their common historical tradition. The O.M. “stamps your life as a recognised success as well as a real one. . . . It is still a very great honour, and the only very great ‘honour.’” He felt strongly about the award because of

my jealousy for the *cause*, the cause of literary and liberal history, so long aspersed and perhaps now coming back to a *little* of its own. It is curious how *one* our feeling is about each other, although I am so much younger and so inferior a writer: the fact that we pursue the same craft and almost alone of men pursue it—I mean literary history of the Macaulay tradition—has brought us into a queerly “equal” intimacy for a father and son. I am always fully alive, even in the secrecy of my own heart, to my inferiority to you as a writer; but still we are both out after the same game, and nobody else is. . . . It encourages me to think that “the homely slighted shepherd’s trade” of literary history is valued by a portion of the world still, and is worth giving one’s life to. Sometimes one is tempted to wonder whether anything but politics and realistic novels and journalism has any influence on the present age. This “honour” helps me to believe that other things count too, indirectly at least, as influences on the makers of opinion at least.²

Born on February 16, 1876, near Stratford-on-Avon, into one of the leading Liberal families, G. M. Trevelyan was surrounded by the dazzling members of the new intellectual aristocracy dominating late Victorian Britain. His birth took place a fortnight before his father, a Liberal M.P. and future cabinet minister, published his biography of the baby’s great-uncle, Lord Macaulay. And G. M. Trevelyan had “the reflex honour of being christened George Macaulay.”³ He was born at Welcombe, the large house and estate owned by his maternal grandfather, Robert Needham Philips, a wealthy Lancashire merchant and a Liberal M.P. for Bury. Welcombe—now a hotel owned by British Rail and catering to tourists visiting Shakespeare’s birthplace—was built by GM’s great-uncle, Mark Philips, one of the first two M.P.’s elected for Manchester after its enfranchisement in the Reform Bill of

² GMT to G. O. Trevelyan (hereafter GOT), June 24, 1911, GOT Papers, box 13.

³ GMT, *Sir George Otto Trevelyan: A Memoir* (London, 1932), 95.



Fig. 1. Sir George Otto Trevelyan, Caroline, and sons (from left) Charles, George, and Robert. Taken at Wallington, the family home, ca. 1896. Courtesy Master and Fellows of Trinity College, Cambridge.

1832. Though Trevelyan's parents used Welcombe as a sort of winter resort after they inherited it in 1890, they looked upon Wallington, the Trevelyan family estate in Northumberland, as their real home. Upon the death in 1886 of the young George's paternal grandfather, Sir Charles Edward Trevelyan, founder of the civil service system and administrator for Ireland and India, G. O. Trevelyan inherited the baronetcy and Wallington where the ten-year-old could imbibe the stable English country life he would come to love. He soon traced his father's steps to Harrow and Trinity College, Cambridge. Like fellow Harrovian Winston Churchill, he was superbly tutored in history and acquired a mastery of English. At Trinity G. M. Trevelyan stood in the shadow of his great-uncle Macaulay, then under attack by Sir John Seeley as unscientific. Lord Acton, Seeley's successor in 1895 as Regius Professor, enchanted the young Trevelyan with his great wisdom and his belief in moral judgment and individual liberty.

From his earliest years Trevelyan was conscious of being in the Macaulay tradition. "I have been reading some chapters of Gibbon for tripos purposes," he wrote his father. "The thing that struck me was that his antitheses were distinctly a foretaste of Macaulay. Also his habit of bringing up historical parallels to illustrate and enforce. It is not done nearly so well, but it is the same thing."⁴ At about the same time, he told his mother: "There is nothing which gives me the feeling of the 'Romance of history', like the Odyssey, though Macaulay's Lays are a splendid effort to fill the place of an Odyssey for the Roman world."⁵ Yet a few years later, after completing his first book, *England in the Age of Wycliffe* (1899), he remarked to his brother Robert Calverley: "I always read Ruskin while I am writing to prevent falling into Macaulayese."⁶ At the peak of his career, he wrote a book on the period of Macaulay's history entitled *The English Revolution, 1688-1689* (1938) and summed up his relationship with Macaulay. Trevelyan was not attempting to rival Macaulay in his current book and purposely avoided the "highlights" of the Glorious Revolution: "I was anxious to strike a different note from Macaulay, to secure confidence for my general views, which as you see are not really very different from Macaulay's after all! For on the big impersonal issues he was right."⁷

The young G. M. Trevelyan was also sensitive to the religious heritage that came down from his grandfather Sir Charles Edward Trevelyan, a devout evangelical. The grandson became an agnostic as he read Ernest Renan's *Vie de Jesus* with a "devouring eagerness," and he told his oldest brother, Charles, that democracy was "all in all my religion, whereby I live and move and have my being. . . . Democracy is I think the corollary of the teaching of Christ."⁸ But GM believed that he had inherited something of his grand-

⁴ GMT to GOT, Aug. 10 [ca. 1893], GOT Papers, box 16.

⁵ GMT to his mother, n.d., GOT Papers, box 15.

⁶ GMT to Robert Calverley Trevelyan (hereafter RCT), July 9, 1900, RCT Papers, Trinity College, Cambridge, box 14, no. 43.

⁷ GMT to RCT, Oct. 18, 1938, *ibid.*, no. 141.

⁸ GMT to Charles Philips Trevelyan (hereafter CPT), July 19, 1893, quoted in Owen Chadwick, *Freedom and the Historian* (London, 1969), 29. See Mary Moorman, "The Youth of an Historian: George Macaulay

father's puritanical moral feeling. He confessed to Charles that "we inherit the moral stamina produced in grandpapa by religion or otherwise, and apply it straight off to our infidel sense of duty. I am now inclined to think that Oliver [Cromwell] and Grandpapa Trevelyan both would have had the same moral stamina even if they had not got it in the form of religion."⁹ G. M. Trevelyan's "moral stamina" took him in his student days on a far different course from his grandfather's on the subject of Ireland: Charles Edward, as virtual head of the Treasury, had shaped Irish policy toward compelling a backward people, as he saw them, into the severe economic virtues; the young George, in supporting Irish Home Rule in 1893, ended letters to his brother Charles with "God Save Ireland." As democracy became the undergraduate's religion, the concluding expression became "God Save the People" or more often "G.S.T.P." or "Ich dien"—"I serve"—the old motto of the Black Prince.¹⁰ GM contrasted the restrictiveness of doctrinaire Christianity with the openness of skepticism.¹¹

After winning a First in the History Tripos of 1896, Trevelyan began to think about a dissertation for a fellowship. He considered the fourteenth century a turning point in English history in the sense that modern ideas were beginning to emerge in the great minds of the day. He had read Stubbs at Harrow and was influenced by the bishop's progressive Whig view of medieval England.¹² And the feeling for great issues and their moral significance that the Liberal Catholic historian Lord Acton had imparted to his student fixed itself on the events of the fourteenth century. Wycliffe and the Lollards represented the struggle for liberty of thought against the power of the Church over men's minds; the Peasants' Rising of 1381 bespoke the rise of English national consciousness. "I am beginning to study the state of the Church," he wrote. "It will be necessary to be very careful and not state anything on hearsay as it is and always will remain a touchy subject." And he told his brother Charles who was then on the London School Board: "It is hard inch to inch fighting with the subject for I am the first that ever compared the authorities in detail, and the damned fools disagree as if they were on the school board. If only some bloody ass of a monk had known what trouble he would give by sticking down the first thing he heard, he might have taken a little more trouble." In February 1897 Trevelyan read a "slice" of his dissertation to the Trinity Historical Society, with Lord Acton, F. W. Maitland, and Archdeacon William Cunningham present. Acton was greatly

Trevelyan, Part 3: Last Years at Harrow," *Contemporary Review*, 225 (1974): 16–17. In 1893 G. M. Trevelyan wrote that St. Francis of Assisi was "a true *democrat* in the highest sense of the word—the first person since Christ was crucified who *really* understood that true Christianity was the gospel of the poor." Until Christianity "could be freed from its trappings," G. M. Trevelyan's religion would be "the only thing I can believe in for certain—the progress of the human race." Mary Moorman's article, "The Youth of an Historian," has four parts in the *Contemporary Review*, 224 (1974): 246–50, 299–305; and 225 (1974): 14–19, 90–97. See also GMT, *De Haeretico Comburendo; Or, The Ethics of Religious Conformity* (Cambridge, 1914).

⁹ GMT to CPT, n.d., in Chadwick, *Freedom*, 29–30.

¹⁰ Chadwick, *Freedom*, 14; Moorman, "Youth of an Historian, Part 3," 225 (1974): 95.

¹¹ GMT to CPT, Oct. 13, 1902, in Chadwick, *Freedom*, 30–31.

¹² GMT to GOT, n.d., GOT Papers, box 16.

impressed and advised George to continue until he had a publishable book.¹³ Awarded a Trinity Fellowship in 1898, Trevelyan published his dissertation as his first book, *England in the Age of Wycliffe*.

Wycliffe is Whig history at its best, viewed through a self-confident Liberal faith. The exuberance of its style, "the lights too high and the shadows too deep,"¹⁴ is reminiscent of Macaulay; yet the work is remarkable for the smoothness of the narrative and for the crisp analysis of motives. In keeping with classical Whig tastes, the book is decidedly anti-Roman and has an air of present-mindedness.

During his first decade of writing, critics would attack Trevelyan's books for their Whiggishness. One reviewer mentioned his "Macaulayisms," his inaccurate attempts to find modern equivalents: John of Gaunt "at the head of a . . . hierarchy of knaves" is compared in *Wycliffe* to an American "political boss" who secures control of the Privy Council, where the "big deals" are made.¹⁵ In *Garibaldi's Defence of the Roman Republic*, complained another critic, Trevelyan's "unbounded enthusiasm" for the Risorgimento prevented him from doing justice to the papacy. His reliance on anticlerical sources led him to write of "the Neapolitan gang" of cardinals and monsignors who surrounded Pope Pius IX.¹⁶ Another example of presentism that a reviewer found trying was Trevelyan's snide allusion to the "dull Germans" who laboriously manufactured a theology out of Luther's writings.¹⁷ And the same commentator skeptically noted the Whiggish optimism of some propositions in *England under the Stuarts*. The "laws of the Cavalier Parliament, which were meant to dragoon all England into one religion," Trevelyan had written, "have helped to secure freedom for a hundred religions, and a thousand ways of thought"; and during Queen Anne's reign "it was the good fortune of England to get all that was good out of both parties, when a few turns of chance would, as often happens, have given her all that was worst."¹⁸

In private GM could also be exuberantly self-confident. His dining table at Trinity, he wrote to his father, consisted of G. E. Moore, "the philosopher," C. R. Buxton, "who travelled far," and Trevelyan, "who is all things to all men."¹⁹ Generally, however, he was more self-critical and skeptical. In a moving letter to his father during the Boer War, he wondered how to describe the British world at the turn of the century:

¹³ G. M. Trevelyan letters in the possession of Mary Moorman, quoted in her "Youth of an Historian, Part 4," 225 (1974): 92-93.

¹⁴ Review by James Tait in *English Historical Review*, 15 (1900): 161.

¹⁵ Review by George Kriehn in *AHR*, 5 (1899-1900): 121, n.2. See GMT, *England in the Age of Wycliffe* (London, 1899), 10.

¹⁶ Review by H. Nelson Gay in *AHR*, 14 (1908-09): 135. See GMT, *Garibaldi's Defence of the Roman Republic* (2d ed.; London, 1910), 87.

¹⁷ GMT, *England under the Stuarts* (London, 1904), 153, quoted by Wilbur C. Abbott in *AHR*, 11 (1905-06): 380.

¹⁸ GMT, *Stuarts*, 469, 346, quoted in *ibid.*

¹⁹ GMT to GOT, n.d., GOT Papers, box 15. George Edward Moore, O.M. (1873-1958) is known for a theory of ethics described as ideal utilitarianism. He was professor of philosophy at Cambridge from 1925 to 1939. Charles Roden Buxton (1875-1942) served as private secretary in 1897-98 to his father, Sir Thomas Fowell Buxton, 3d Bart., when he was governor of South Australia. Charles later served as principal of Morley College and editor of the *Albany Review*; he was also a diplomat and Labour M.P.

The British world is so immense, the multitudinousness and infinite complexity of things is so vast that no man can really judge it, especially by historical analogy. I do not pretend to judge it, or to know whither—in this vast twilight or this vast grey-dawn of humanity—we are groping, some of us with passionate greed and some of us passionate self-sacrifice, all blindly enough. The general criminality has to be much clearer than it is in England before any man can say for certain—it is getting worse. . . . you regarded the Crimea when it was on *not* from Bright's standpoint and so did not see what very likely was worse and more universal folly and lying. Now you regard this war from another point of view and see it all. . . . I am only asking you to be uncertain that we are going down hill. You may or may not be as clear-sighted as anyone; I certainly do not pretend to be more clear-sighted. I only mean that we are all moles—though of course we must all go on driving our little tunnels as straight as we can and with all our strength, until the mole-catcher come. But if we are moles in vision, the best of us become gods in spirit.²⁰

Suddenly in 1903 Trevelyan decided to resign his fellowship and leave Cambridge. The reason he gives in his autobiography is that he could write literary history “in more spiritual freedom away from the critical atmosphere of Cambridge scholarship.”²¹ The fields of history upon which Cambridge historians were concentrating—economic, diplomatic, and constitutional—offered little attraction to Trevelyan, who reveled in the drama of history. John Bagnell Bury, upon succeeding Lord Acton as Regius Professor of Modern History at Cambridge in 1902, delivered an inaugural lecture entitled “The Science of History.” Shortly after leaving Cambridge, Trevelyan published his polemical reply in an essay, “Clio, A Muse.” He argued that history was both an art and a science; defining three distinct functions of history, the scientific, the imaginative, and the literary, he stressed the literary function.²² He expressed his personal reaction to the “scientific” approach in a letter to his parents: “The way you feel a thing is much more important than the complete truth of the thing you feel. Perfect wisdom be damned. I’ve at present no pretensions to it.” And in a line reminiscent of a Macaulay confession: “Besides, if you are writing about the past, is it not artistically fortunate to be out of love with the present?”²³ In 1907 George lamented to his brother the decline of literary history: “But in the present state of history in England, a history book is treated as an historical monograph, and consigned to ‘historical students’ unless it violently proclaims that it regards itself also as literature and appeals to the general public.”²⁴ Yet by 1915 he could write to his father from the University of Michigan: “I find that your books and mine are held in remarkably high esteem; and the ‘scientific history’ idea, in its narrowing and exclusive sense, is quite over.”²⁵

²⁰ GMT to GOT, n.d., *ibid.*

²¹ GMT, *An Autobiography and Other Essays* (London, 1949), 21. According to Chadwick, *Freedom*, 20–21, G. M. Trevelyan thought of history in terms of its “service to humanity.” In 1904 he helped found the *Independent Review*, and he went to London to be involved in Liberal politics and “to be at the centre of the literary world, and not because he wished to run away from men doing fundamental research.”

²² GMT, *Clio, A Muse and Other Essays Literary and Pedestrian* (London, 1913), 30–31.

²³ GMT to his parents, n.d. [190–?], GOT Papers, box 15.

²⁴ GMT to RCT, Apr. 13, 1907, RCT Papers, box 14, no. 68.

²⁵ GMT to GOT, May 6, 1915, GOT Papers, box 14.

Away from the restrictive influence of academe, Trevelyan no longer had to be concerned about the "scientific" historians, such as the reviewer of his first book who accused him of being "over eager to publish a piece of work of ambitious scope" when he would have been better advised to begin with an "exhaustive" study of the Rising of 1381.²⁶ He turned to literary history, agreeing to write the volume on the Stuart period for Methuen's History of England series. *England under the Stuarts* (1904) became the most popular volume in the collection, going through some twenty-two reprintings, though its reception among professional historians was cool.²⁷ Certainly Trevelyan's decision to leave academic life had been justified.

In 1904 G. M. Trevelyan married Janet Penrose Ward, a daughter of Mrs. Humphry Ward, the Victorian novelist and social worker, and a grandniece of Matthew Arnold. Their wedding presents included, fortuitously, a collection of books on Italian history, among them Garibaldi's memoirs. In the story of Garibaldi, Trevelyan would find a subject that suited his genius.

Trevelyan began his Garibaldi trilogy about the time of the greatest Liberal election victory in a generation of English politics. The reform-minded government was at the crest of its popularity when the first volume, *Garibaldi's Defence of the Roman Republic*, appeared in 1907. And the public mood was receptive to a heroic account of the struggle for liberal humanism. The response to this romantic treatment of the Risorgimento was enthusiastic, and popular demand rose for the remaining volumes, *Garibaldi and the Thousand*, published in 1909, and *Garibaldi and the Making of Italy*, in 1911. Trevelyan was not finished with Italy, for during the Great War he served as the commandant of the British Red Cross Ambulance Unit there and worked with the Italian army on the Isonzo and Piave fronts. His last book on Italian history, *Manin and the Venetian Revolution of 1848*, published in 1923, was less enthusiastically received than the Garibaldi books. It was a more cynical time, this postwar year in Britain; and besides, the subject may have ill fitted its author. His student, J. H. Plumb, observes in an essay on Trevelyan that Venetian society, "as far gone in decay as it was developed in sophistication, was not a world for the great simplicities of Trevelyan's heart and mind."²⁸

The Garibaldi saga, however, ranks "with the works of Prescott or Parkman, in fact with the world's best narrative histories."²⁹ The simple, epic quality of Giuseppe Garibaldi and his Brazilian-born wife Anita made them ideal characters for Trevelyan's literary talents. His Garibaldi trilogy has its weaknesses in failing to explore the economic and social causes of Italian nationalism, and in assessing the papacy and the French too harshly and the British too leniently. It remains an evocation of liberal idealism and hope.

²⁶ Review by James Tait, 164.

²⁷ See the short notice by C. S. Terry in *English Historical Review*, 20 (1905): 403-04.

²⁸ J. H. Plumb, *G. M. Trevelyan* (London, 1951), 21. See also Henry R. Winkler, "George Macaulay Trevelyan 1876-," in S. William Halperin, ed., *Some 20th-Century Historians: Essays on Eminent Europeans* (Chicago, 1961), 31-55, and G. Kitson Clark, "G. M. Trevelyan as an Historian," *Durham University Journal*, 55 (1962): 1-4.

²⁹ Plumb, *Trevelyan*, 21.

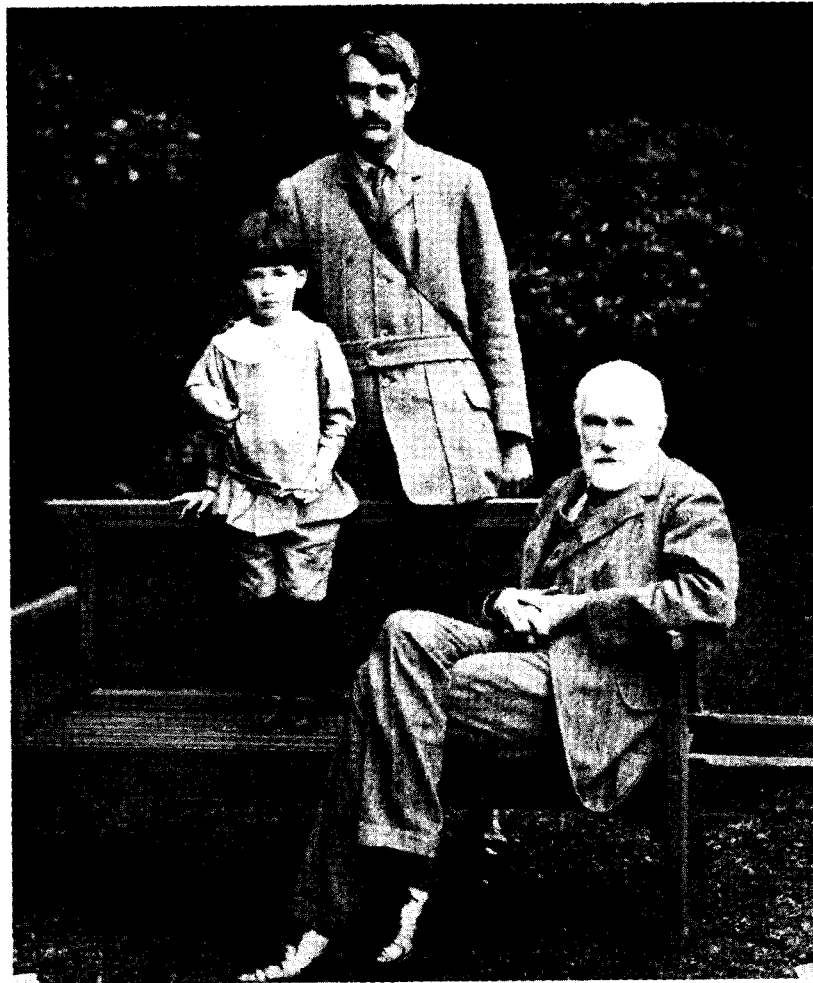


Fig. 2. Sir George Otto Trevelyan (seated) with his son George Macaulay and grandson Theodore Macaulay (1906-11); Wallington, 1910. Courtesy Mrs. Mary Moorman, Bishop Mount, Ripon, North Yorkshire.

Trevelyan wrote in his introduction to the second volume in 1909: "To my mind the events of 1860 should serve as an encouragement to all high endeavour amongst us of a later age, who, with our eyes fixed on realism and the doctrine of evolution, are in some danger of losing faith in ideals, and of forgetting the power that a few fearless and utterly disinterested men may have in a world where the proportion of cowards and egoists is not small."³⁰

For the romance of history, with a dramatic narrative that is attentive to detail but inspired with poetry, the Garibaldi trilogy is a colorful, powerful example. Trevelyan wrote of the Roman scene on the eve of the city's fall to the French in 1849:

The night of June 29-30, the Feast of St. Peter and St. Paul, was selected by Oudinot for the final assault. During the earlier part of the night the *festa* was celebrated in the

³⁰ GMT, *Garibaldi and the Thousand* (London, 1909), 7.

town in right Roman fashion, with lighting of candles in the windows, and sending up of rockets in the streets—functions which that mercurial people would not forego even under the shadow of impending doom. The Triumvirate gave official countenance to these mild *circenses*, and the dome of St. Peter's blazed with every extravagance of colour. The French officers, as they stood in front of their dark columns, waiting for the signal to mount the breach, saw below them the holy city glowing "like a great furnace, half-extinct, but still surrounded by an atmosphere of fire." Suddenly the heavens were opened in wrath, and a deluge of rain fell on the disobedient children of the Pope, extinguishing their last poor little fires of joy. When the torrential storm had passed away, one light alone, from the top of the great dome of St. Peter's, still shone through the thick darkness, beckoning the crusaders to the assault.³¹

Gripping though the romance of the story might be, Trevelyan pursued analysis as well as narrative, and his sense of the ironic in history would be an antidote to any facile liberal optimism. The story of Italian "deliverance by the wisdom of Cavour and the valour of Garibaldi will remain with mankind," he remarked, "to warn the rash that the brave man, whatever he and his friends may think, cannot dispense with the guidance of the wise—and to teach the prudent that in the uncertain currents of the world's affairs, there come rare moments, hard to distinguish but fatal to let slip, when caution is dangerous, when all must be set upon a hazard, and out of the simple man is ordained strength."³²

AS HE WAS WRITING about various struggles for individual and national liberty, Trevelyan responded in a predictable Liberal fashion to current events. The Dreyfus affair captured his soul. In 1898 he reported that he was "profoundly interested" in "the coming struggle in France, which cannot be long delayed now. If the man gets out I shall burn a bonfire in my heart. May France live!"³³ Eight years later, while writing the first volume on Garibaldi, Trevelyan celebrated the final exoneration of the innocent man: "Dreyfus reestablished justice on earth, in spite of the Pope and all his legions of evil spirits," he wrote his mother. "It is a glad morning for it, and the sun shines on the river. . . . The exculpation of Dreyfus is one of the noblest stories in the history of mankind."³⁴ With his admiration for Émile Zola,³⁵ Trevelyan undoubtedly would have written about the struggle in France were it not for his aversion to writing histories of his own time.

Trevelyan's hesitancy to write on current affairs, which distinguishes him from his father and granduncle, was based on his belief that he had no understanding of his own era.³⁶ In 1913 his brother Robert, a Cambridge classicist, suggested—facetiously, perhaps—that George write a history of the current Balkan wars. He replied, "I do not think this is a case for me to try a

³¹ GMT, *Garibaldi's Defence*, 217–18.

³² GMT, *Garibaldi and the Thousand*, 7.

³³ GMT to RCT, Dec. 31, 1898, RCT Papers, box 14, no. 28.

³⁴ GMT to his mother, July 13, 1906, GOT Papers, box 15.

³⁵ See GMT to GOT, n.d. [ca. 1893], *ibid.*, box 16.

³⁶ GMT to RCT, Oct. 27, 1926, RCT Papers, box 14, no. 107.

'contemporary history'. After all good contemp. histories like Thucydides, Clarendon and even Burnet were written by men who lived in the affair and were natives. I know nothing about the Balkans and know no Balkan language. And the Balkan peoples *all* lie like [the] Devil."³⁷

To the dedicated English Liberal, autocratic Russia symbolized feudal tyranny. Trevelyan exulted in the revolutionary situation of 1905–06 but like a good constitutional Whig worried about the weakness of the center.

If one could be sure of the result, one would welcome the struggle however sanguinary. But it seems extremely doubtful, the more so because the Duma had not sat long enough to make itself an authority of great power, and one fears the revolution will have no centre, and will run to extremes and divisions. . . . But however many generations it may take, Russia will be free some day . . . [and] will be far more socialistic than any of us. The peasant will brook no Lord, for he is many and strong, unlike the English labourer.

If Trevelyan were unmarried, he told his mother, he would like to fight for the Russian liberals.

The amount of blood that will be shed will be frightful. The Italian and French Revolutions will be child'splay to it. They care nothing for their lives at ordinary times, and in these occasions "which turn the sluggard's blood to flame, the coward's heart to steel"—less than nothing. They are crossed with the peoples of the east who think life of little worth at best. But if their revolution can be carried out, perhaps life will become of more value over there, in a few thousand years. . . . I am afraid the Russians won't produce a man as attractive as Garibaldi,—at least not as attractive to us Westerns.³⁸

The March Revolution of 1917 found Trevelyan with his Red Cross unit in Italy. Though disappointed that the Russians were not Whigs, he nevertheless saw democratic progress:

My comparison of the Russian Revolution to 1689 was disproved 2 days after I had written it. The DT [*Daily Telegraph*] is particularly good on Russia and remarkably sympathetic even with the extreme forms of democracy there, which other papers do not try to understand at all. I wish the Russians were Whigs, but as they aren't one must hope for the best to happen in some other odd, new democratic way. The dangers are too obvious to remark on. But *in any case* I believe democracy is on the march now all along the line.³⁹

But with the triumph of the Bolsheviks, Trevelyan almost despaired of the future of Europe. He told his father that if the U.S. Senate "throws out" the League of Nations treaty "there will be Bolshevism and war all over Europe for a generation to come, while America smugly retires into her shell." Without American influence in Europe as a check against Communism, "we are all undone."⁴⁰

Perhaps it was extremism abroad and at home that turned Trevelyan back

³⁷ GMT to RCT, July 30, 1913, *ibid.*, no. 89.

³⁸ GMT to his mother, July 23, 1906, GOT Papers, box 15.

³⁹ GMT to his mother, Apr. 8, 1917, *ibid.*, box 14.

⁴⁰ GMT to GOT, Feb. 25, 1919, *ibid.*



Fig. 3. From left, Charles Philips Trevelyan, Robert Calverley Trevelyan, George Macaulay Trevelyan, with Sir George Otto Trevelyan in foreground. Ca. 1910. Courtesy Master and Fellows of Trinity College, Cambridge.

to his own political heritage for his next two books. His *Life of John Bright* appeared a year before the Great War, and *Lord Grey of the Reform Bill* two years after the Armistice. Both Trevelyan and his father stood in the Radical-Liberal tradition of Bright, and his mother's family had been associated with the free trade movement in Manchester headed by Bright. Trevelyan successfully conveys Bright's great moral power and independent drive that pricked the conscience of the Whig-Liberal party for half a century. Fixing almost exclusively on Bright, Trevelyan slights his opponents and oversimplifies the political difficulties confronting Sir Robert Peel. Bright, the pacifist reformer, would serve as a powerful example to disillusioned Liberals in the chaotic year before World War I. And Trevelyan's oldest brother, Charles Philips Trevelyan, parliamentary secretary to the Board of Education, was to resign from the Liberal government in August 1914 as a protest against Britain's entry into the Great War.

While sympathetic to the pacifism of his two older brothers, George adopted an American political term, "mugwump," to describe his realistic, Whiggish position on the war. He wrote that he was "really too broken in spirit" to answer his brother Bob's letter properly; "and indeed the mugwump is not morally in a position to hold his own against either side. He feels a self contempt when he compares himself to his wholehearted friends." He confessed that "one feels it hardly matters what one does or thinks in this doomsday." In fact, the war was "far the greatest catastrophe in modern history." However, he reminded his brother: "But as long as you have devils like the Russian and German militarist parties in being and in *power*, can the rest [of] the world adopt complete pacifism and survive?"⁴¹

Lord Grey's career, on the other hand, would serve as a reminder of the Whig tradition to postwar cynics. Regarding *Lord Grey of the Reform Bill*, G. M. Trevelyan wrote in his short autobiographical essay: "The theme of glorious summer (in this case the summer of Reform) coming after a long winter of discontent and repression, is, as I have said, congenial to my artistic sense. And then the background of Grey's life was my own—Northumberland."⁴² Because of his Whig heritage, Trevelyan glossed over complexities and demonstrated little sympathy with the short-sighted Tories who had stalled reform for so long. And inferentially the biography was a polemic against the demise of Liberalism he was witnessing after Versailles.

Lord Grey of the Reform Bill was condemned at the time as an indictment of Tory administration "conceived in the unmeasured violence of a political antagonist,"⁴³ and even friendly critics have rated the biography as perhaps Trevelyan's poorest book.⁴⁴ Still, it constitutes a philosophical statement of the case for a rational and temperate political process. "In an age when the law of perpetual and rapid change is accepted as inevitable, and the difficulty

⁴¹ GMT to RCT, Aug. 28, 1914, RCT Papers, box 14, no. 204.

⁴² GMT, *Autobiography*, 34.

⁴³ Review by C. E. Fryer in *AHR*, 26 (1920-21): 91.

⁴⁴ See Plumb, *Trevelyan*, 22-23, and Winkler, "Trevelyan," 44-45.

is to obtain progress without violence,” Trevelyan wrote in the preface, dated December 1919, “there may be profit in the story of a statesman who, after a period of long stagnation and all too rigid conservatism, initiated in our country a yet longer period of orderly democratic progress, and at the critical moment of the transition averted civil war and saved the State from entering on the vicious circle of revolution and reaction.” Though lacking the genius of Gladstone, Lord Grey succeeded “more completely” than the Grand Old Man and lacked his faculty “for making mistakes.” To understand Grey’s “paramount influence” in Britain between 1830 and 1832 “is to understand the position and outlook of those liberal-minded aristocrats peculiar to the history of our island, whose existence was one of the reasons why the political traditions and instincts that the English have inherited differ so profoundly from those of Germany, of France, or even of Austria.”⁴⁵ Trevelyan reaffirmed his faith in English constitutional progress through enlightened Whig-Liberal leadership.

In the Bishop Creighton Lecture delivered at the University of London in 1919, Trevelyan made even more explicit his faith in the English heritage and traditions. The age of “unrest” after the Great War inspired his “hope that we in this island shall not lose too much of our moral and intellectual inheritance from the past.” Englishmen would never forget, he wished, “that we are ‘of history’s blood royal,’ English of the old stock, with the greatest record of ordered progress in the world.” Here is a remarkable anticipation of the Churchillian rhetoric of World War II and the “chosen people” theme that would permeate Trevelyan’s *History of England*. He was certain that World War I would produce a new way of looking at the past: the American Revolution, the French Revolution, Lincoln, and Bismarck would all be viewed differently after the Great War. He argued that the militarists had “overshot their mark” because by making war “universal instead of professional” they had rendered it “unendurable.” In a burst of Wilsonian idealism he exclaimed, “Military despotism has been slain. . . . Free will, men’s moral choice in national and international affairs, has returned to earth.”⁴⁶

Trevelyan’s Whiggish elitism and supreme confidence in the superiority of English institutions and traditions were further confirmed by the fascist coup of 1922 in his second country, Italy. Frightened by the Bolshevik threat in Europe, Trevelyan also criticized Mussolini’s fascism. But in a lecture at Oxford in 1923 the leading Whig historian presented a sympathetic portrait of Il Duce. “Signor Mussolini,” he remarked in concluding the lecture, was “a great man and, according to his lights, a very sincere patriot.” Trevelyan offered a prayer for Mussolini “not that he victoriously destroy free institutions in Italy, but that he may be remembered as a man who gave his country order and discipline when she most needed them, and so enabled those free institutions to be restored in an era happier than that in which it is

⁴⁵ GMT, *Lord Grey of the Reform Bill* (London, 1920), viii, 368.

⁴⁶ GMT, *The War and the European Revolution in Relation to History* (London, 1920), 8, 12–13, 44, 40–41.

our present destiny to live." Trevelyan reminded his audience that parliamentary government was exported from England to Italy and since Cavour's death had enjoyed no great Italian exponent. "The Italian race has in its blood . . . the politics not of the lobby or of the polling booth, but of the piazza." While the English were putting a Conservative government in power by holding a general election in 1922, "the Italians achieved a similar object by a series of rows in the piazza all over Italy, culminating in a grand national 'row in the piazza'—Signor Mussolini's march on Rome." It was difficult for Italians to appreciate general elections, Trevelyan complacently explained, for they were not "to the manner born. We have this obscure inherited instinct. The Italians have it not. In England a general election is a moral earthquake. . . . But in Italy a general election is the sum of a number of obscure intrigues." Trevelyan was more concerned about freedom of speech than parliamentary government—"perhaps because I am so academic." He contrasted the English temper to that of the fascists, who "regard the suppression of free speech not as one of Cromwell's 'cruel necessities', but as a thing good in itself, an ideal realized." Yet Trevelyan was even more critical of the Italian Left. He denounced the antipatriotic terror after the war and the "defeatist" assaults on men in uniform and on capitalism and property of all sorts. "Red rowdyism" encouraged "young ex-service men in football shirts"—the private fascist army—"to fight the Communists and defeatists with their own weapons." Again Trevelyan could moralize to the advantage of his fellow countrymen: "It was as natural for the Italian socialist to terrorize his fellow citizens as for the English socialist to walk to the polling booth. History can best tell you why."⁴⁷

During the early twenties much of Trevelyan's energy was spent on public work, as a member of the Royal Commission on Oxford and Cambridge and an active supporter of the National Trust. His extensive research on Grey and Bright, however, naturally led to a textbook, *Britain in the Nineteenth Century* (1922). More temperate in its Liberal bias than the study of Grey, it vividly portrays the history of England from the days before the French Revolution to 1832. After the Reform Bill of 1832 it becomes "a conventional political account," Henry Winkler has commented, and the period after 1870 is treated superficially. The work is chiefly remembered for its literary style and for the good introduction it provides to the century.⁴⁸ Its success, however, gave Trevelyan and his publishers an idea—a new single-volume history of England to replace the last famous one, J. R. Green's *Short History of the English People* (1874).

In 1924 Trevelyan was invited to Harvard University to deliver the Lowell Lectures. They formed the basis of his *History of England*, published in 1926, which soon became his most popular work and "could only have been written by a liberal and a humanist."⁴⁹ It stresses the genius of the English people for

⁴⁷ GMT, *The Historical Causes of the Present State of Affairs in Italy* (London, 1923), 20, 16, 8, 17–18, 15.

⁴⁸ Winkler, "Trevelyan," 45–46.

⁴⁹ Plumb, *Trevelyan*, 25.

compromise, civil liberty, and social justice. "In answer to the instincts and temperament of her people," Trevelyan wrote in his introduction, England "evolved in the course of centuries a system which reconciled three things that other nations have often found incompatible—efficiency, popular control, and personal freedom."⁵⁰ Herein we see the major problem of the book, especially since it was so widely read for its vivacious style and brilliant epigrams. E. P. Cheyney warned in a review that the survey was too ardently nationalistic—"not simply with that gentle interest in his own people and country which suffuses Green's work . . . but with a more militant assertive sense of the superiority of English institutions and character over those of other nations that savors of the later nineteenth and early twentieth centuries—the nationalism of the days before, during and to a lesser extent perhaps since the Great War." Cheyney objected to Trevelyan's portrayal of the English as a "chosen people" among nations and worried that English readers would become self-satisfied, complacent, and blind to their own defects.⁵¹ Trevelyan had written a Whig history of England par excellence. It both reflected and shaped national awareness among the English of their own history. Whig history had become the official consensus history.

Trevelyan's *History of England* "has—although one might almost say 'Thank God!'"—a bias, it is frankly liberal and protestant," writes Plumb.⁵² This very "tendency in many historians to write on the side of Protestants and Whigs" the young Herbert Butterfield would denounce a few years later in *The Whig Interpretation of History*.⁵³ Critics of the Whig view, like Butterfield, overlook the most vulnerable point for Whig history—the Irish Question. Trevelyan, like Macaulay, denounced the brutality of the Irish policies during the seventeenth and eighteenth centuries, but in the *History of England* the Liberal who had once rejoiced in the coming of Irish Home Rule showed no sympathy for modern Irish nationalism.

Certainly events during the Great War and the accompanying Anglo-Irish War had changed Trevelyan's outlook. Writing in the mid-twenties, he was merely sanctioning the Irish settlement of 1922. Yet his treatment of Ireland for the years after 1870 has a note of smug superiority. In 1885, he remarked of Charles Stewart Parnell, "The balance of power at Westminster lay in the hands of a strange man who, though himself of Anglo-Saxon origin, regarded British Liberals and Tories with a cold, indifferent hatred."⁵⁴ In later editions of the one-volume history he hardly sounded like a dedicated Home Ruler. Describing events after 1912, Trevelyan wrote that the Conservative party, led by Bonar Law, "openly encouraged Ulster to arm for resistance under Sir Edward Carson's sincere and dogged leadership." The Liberal government, in Trevelyan's view, "ought to have announced that the six Protestant counties of Ulster should be allowed to contract out from Home Rule." And

⁵⁰ GMT, *History of England* (London, 1926), xvii.

⁵¹ Review by E. P. Cheyney in *AHR*, 32 (1926-27): 571-72.

⁵² Plumb, *Trevelyan*, 24.

⁵³ Herbert Butterfield, *The Whig Interpretation of History* (London, 1931), v.

⁵⁴ GMT, *History of England*, 680, 687.

perhaps adopting a more realistic viewpoint to suit his optimistic assessment of the British establishment, Trevelyan reversed his earlier, private support of Gladstone: "Above all, Gladstone's acceptance of Parnell's claim to have Protestant Ulster as a part of the new Ireland, was more than an error in tactics. It flew in the face of racial and political possibilities."⁵⁵

Though the young Harrovian of 1892 had been a strong supporter of Gladstone's Home Rule bill and thought its passage would enable the Liberal prime minister to "end his days with the greatest reputation ever possessed by a living statesman,"⁵⁶ after World War I the GOM was no longer a hero to the mature Trevelyan. And becoming an established historian apparently made Trevelyan more sensitive about his family's role in Irish history. Writing in 1932 about his grandfather, Sir Charles Édward Trevelyan, he curiously failed to make a single reference to Sir Charles's role in administering relief during the Irish Famine—perhaps an example of the "family piety"⁵⁷ he later excused in Churchill's *Marlborough*. He had read his grandfather's letters, but it was not until 1960 that selections were published from the huge collection of Sir Charles's letters dealing largely with the famine.⁵⁸ These letters were used by Cecil Woodham-Smith in her unflattering portrait of the obstinate and insensitive grandfather.⁵⁹ In 1893 G. M. Trevelyan had compared his grandfather to Cromwell.⁶⁰ Yet in historical works he either covered up or ignored his grandfather's policies toward Ireland—more disastrous than those of Cromwell.

Clearly the Trevelyan family experiences in Ireland had not been happy ones. In the biography of his father, Sir George Otto, G. M. Trevelyan could not avoid recounting the two frustrating years GO had endured as Irish chief secretary from 1882 to 1884—years followed by a decline in his political career. During the Anglo-Irish War, GM would meditate on the effects of delayed emancipation in Ireland: "England herself has the opportunity of learning that lesson afresh every generation in her dealings with Ireland."⁶¹ This was certainly an accurate statement for the generations of his grandfather and father. For his own generation G. M. Trevelyan observed, "Some nations, like the Irish, are *too* historically minded, in the sense that they cannot get out of the past at all."⁶²

THE CHANGE IN TREVELYAN'S ATTITUDE toward the Irish generally reflected his shift toward conservatism, which brought him eventually to incorporate

⁵⁵ *Ibid.* (3d ed.; London, 1945), 707–08, 688.

⁵⁶ GMT to GOT, July 3 [1892], GOT Papers, box 16.

⁵⁷ GMT to the *Times Literary Supplement*, Oct. 19, 1933, quoted in GMT, *England under Queen Anne*, vol. 3; *The Peace and the Protestant Succession* (London, 1934), xi.

⁵⁸ GMT, *Sir GOT*, 14, n.1. See Jennifer Hart, "Sir Charles Trevelyan at the Treasury," *English Historical Review*, 294 (1960): 92–110.

⁵⁹ Cecil Woodham-Smith, *The Great Hunger: Ireland 1845–1849* (New York, 1962), *passim*.

⁶⁰ Chadwick, *Freedom*, 29–30.

⁶¹ GMT, *The War and Revolution*, 41.

⁶² GMT, *Autobiography*, 63.

the Tories into the Whig tradition. He found himself increasingly uncomfortable in writing about the twentieth century and even about Victorian Britain; it is in the Victorian section of his *History of England* that his anti-Irish comments appear. In its 703-page edition of 1926, only eighty-eight pages are devoted to Britain after 1815. Writing to his brother Bob, Trevelyan was worried about the modern section of the book: "I am glad you find the Victorian part was tolerable. The 'Epilogue',—imposed on me much against my will by the Publisher's view of necessity, I believe a correct view,—could not be anything but a blot. I don't understand the age we live in, and what I do understand I don't like."⁶³

It was certainly natural for a man of Trevelyan's views and background to dislike the events of 1926. Fascism and communism were flourishing on the Continent, and England was experiencing the disruptive General Strike. Moderate men would likely support the Tories, and Trevelyan would reinterpret the Whig tradition in his Romanes Lecture at Oxford. Edmund Burke, in Trevelyan's view, was the key figure in the transformation of the Tory party. In his later anti-Jacobin mood, Burke became "the prophet of the Tory Party and of nineteenth-century Conservatism" because he taught the Tories to regard themselves as "the true heirs and protectors of the English Revolution Settlement of 1689 against the false cosmopolitan lights of the French Revolution." The violent methods of the Jacobins and Napoleon "connected modern democracy in its earliest stage not with the ballot-box but with the bayonet." The English Tories then were put "into the advantageous moral position of defenders of law and constitutionalism against the 'direct action' of the Jacobins and the popular Caesarism of Napoleon." After Waterloo, Canning's variety of Toryism with its defense of parliamentary government "became synonymous for a few years with the cause of European liberty." But the Tories failed to adapt to the new industrial age; both Burke and Canning opposed the abolition of rotten boroughs. At this moment the Whig party, which had failed to appreciate the need for war with Napoleon's France, did realize "the necessity of Parliamentary Reform if Parliamentary government was to survive in the new era." No single party could have been right both on the war with France and on the necessity for reform; therefore, the need for a two-party system. When, during the French Revolutionary era, Roman Catholic claims to civil rights were revived, opposition on the part of the Tories completed their "reconciliation . . . to the principles of 1689. The revived Tory party became enthusiastic for the House of Hanover and the Revolution Settlement, while on the other hand the Whigs began to demand civil rights for Roman Catholics, which the Tory party won great popularity by refusing." And the continuity of the two parties was to be found "mainly in the unbroken connexion of the Tories with the Church interest, and of the Whig aristocrats with the Protestant Nonconformist voters." The party system was one of the devices whereby "England learnt to combine efficiency

⁶³ GMT to RCT, Oct. 27, 1926, RCT Papers, box 14, no. 107.

with freedom." Trevelyan concluded "from the vantage point of our own very different age" that the "dualism in the religious life of the nation, reflected . . . into a political dualism, . . . contributed largely to the unexpected success of free Parliamentary government as a method of ruling a great country and a great Empire in peace and in war."⁶⁴ With the demise of the Liberal party after World War I, Trevelyan was determined to point out that the Conservatives were very much in the Whig tradition of 1689.

Perhaps coincidentally but appropriately, the Conservative prime minister, Stanley Baldwin, in 1927 appointed G. M. Trevelyan Regius Professor of Modern History at Cambridge, succeeding J. B. Bury. In 1930 GM came into the honor his father, who died in 1928, had held: he received the Order of Merit, limited to a membership of twenty-four. According to Owen Chadwick, Professor Trevelyan's lectures at Cambridge were not known for their "eloquence and stimulus," but he stood as a symbol to the undergraduates of the tradition of Whig history and of the doctrine that history is enjoyable and indispensable for a humane education.⁶⁵ The appeal of history, Trevelyan said in his inaugural lecture for the Regius Professorship, is essentially poetic, for history deals with the mysteries of life and death and time. "Historians are born, not manufactured," he observed. And he warned the Cambridge scholars: "Today the writers may sometimes be too academic and the readers not academic enough for the purposes of mutual understanding."⁶⁶

With his reputation firmly established, Trevelyan began his magnum opus—the three-volume *England under Queen Anne*, which he later described as "my best work, except perhaps the Garibaldis." In assessing his motivation for writing this major historical work, Trevelyan spoke of the Whig influence his family background exerted on him: "The idea of taking up the tale where my great-uncle's history had broken off, was perhaps a fancy at the back of my consciousness." Lord Macaulay's *History of England* concluded with William III in 1701. But Trevelyan said he was "more seriously attracted by the dramatic unity and separateness" of the period from 1702 to 1714 between the Stuart and Hanoverian eras. "I always liked military history, and the Marlborough wars are one of its greatest themes," he continued; "I always liked Scottish history, and the Union of 1707 was its turning point." Perhaps the most important motive was that a history of Queen Anne's reign would provide a story with a happy ending for a nation needing one at a time of disintegration and depression.⁶⁷

The preface to *Blenheim* (1930), the first volume of *England under Queen Anne*, reads like a message of hope and inspiration to Trevelyan's countrymen during the depression. The Augustan Age was in reality a "five-act tragedy," he reminded them, when "through the crash of each successive crisis of war and politics, the fortune of England moves forward on the tide of destiny." Yet

⁶⁴ GMT, *The Two-Party System in English Political History* (Oxford, 1926), 23–25, 10, 27.

⁶⁵ Chadwick, *Freedom*, 2.

⁶⁶ GMT, *The Present Position of History* (London, 1927), 24–29.

⁶⁷ GMT, *Autobiography*, 45–46, 34.

“what men that little rustic England could breed! A nation of five and a half millions that had Wren for its architect, Newton for its scientist, Locke for its philosopher, Bentley for its scholar, Pope for its poet, Addison for its essayist, Bolingbroke for its orator, Swift for its pamphleteer and Marlborough to win its battles, had the recipe for genius.”⁶⁸ Unlike Macaulay, Trevelyan viewed the duke of Marlborough as an inscrutable hero. With his conviction about the great historical consensus in Britain’s politics, Trevelyan stressed the sincerity of both Whigs and Tories: “Even the lapses of Tory ministers into Jacobite treason,” a reviewer said, “are to him a natural result of the shock of the Revolution to deep-rooted loyalties.”⁶⁹

As he was completing the second volume on the reign of Anne, *Ramillies and the Union with Scotland* (1932), Trevelyan was concerned about its emphasis on military history and its public reception. Thanking his brother Bob for corrections on the manuscript, he remarked, “I have been terrified by the fear of its becoming a ‘drum and trumpet’ history, especially in these anti-war times, and especially as I like drums and trumpets myself so much, provided they were blown a good 100 years or more ago!”⁷⁰ One reviewer noted that in writing about the complicated domestic politics of the era Trevelyan’s interest seemed to flag. The problem was his preference for “moderate men” at a dull time when they had no program except to remain in power.⁷¹ And Trevelyan’s belief in the two-party system distorted his portrait of the Augustan Age. The backstairs politics of Harley and Godolphin, as they sought power at all costs, could not be easily simplified into party struggles.

Overlapping the publication from 1930 to 1934 of Trevelyan’s three-volume *England under Queen Anne*, Winston Churchill’s four-volume biography of his ancestor, John Churchill, *Marlborough: His Life and Times*, appeared between 1933 and 1938. Trevelyan and Churchill, who had been contemporaries at Harrow, were now rival historians in their accounts of early eighteenth-century England. They were in essential agreement in interpretation, even on Marlborough’s character. Trevelyan did respond, though, to Churchill’s calling Macaulay a “liar” for his devastating portrait of Marlborough. Macaulay’s mistaken reading of Marlborough, Trevelyan wrote in a letter to the *Times Literary Supplement*, was the worst thing in his history; it was not surprising that “Mr. Churchill’s family piety . . . aroused him to take revenge.” Trevelyan reminded Churchill that “there is a surface case against Marlborough, and many people in his own day thought ill of him. An historian who, before the days of our modern research, was deceived by these phenomena into thinking Marlborough was a bad man was not necessarily dishonest.” It was ironic, observed Trevelyan, that “Macaulay, for all his alleged Whig bias, trusted far too much to Jacobite and High Tory libels about Marlborough.”⁷² Despite their minor disagreements, Winston Churchill, as prime minister during the grim days of 1940, appointed his

⁶⁸ GMT, *England under Queen Anne*, vol. 1: *Blenheim* (London, 1930), vii.

⁶⁹ Review by Violet Barbour in *AHR*, 36 (1930–31): 379.

⁷⁰ GMT to RCT, Oct. 27, 1932, RCT Papers, box 14, no. 129.

⁷¹ Review by Violet Barbour in *AHR*, 39 (1933–34): 119–20.

⁷² GMT to the *Times Literary Supplement*, Oct. 19, 1933, quoted in GMT, *Protestant Succession*, xi.

fellow Whig historian, G. M. Trevelyan, as master of Trevelyan's revered Trinity College, Cambridge.

The final volume in the Queen Anne trilogy, *The Peace and the Protestant Succession* (1934), celebrated the apotheosis of the Whig interpretation and "the good sense of the English people." Trevelyan argued that English character shaped English politics: "The Englishman is famous for forgetting what is best forgot." England acquired security and liberty under the Hanoverian Constitution because the Whigs under Walpole were moderate men. "The great art of letting your neighbour alone, even if he thinks differently from you, was learnt by Englishmen under Walpole, at a time when the lesson was still a strange one elsewhere." Disraeli had called the rule of the great Whig families under the first two Georges "the Venetian Oligarchy," but Trevelyan criticized the regime on different grounds: "Not tyranny but an exaggerated conservatism was the weakness of the Walpoles and of the Pelhams after him. *Quieta non movere* ['Let sleeping dogs lie'] is not the motto of a Reformer, but neither is it the motto of a Tyrant." Because England between the Glorious Revolution and the death of George II established the rule of law, nineteenth-century England witnessed change through peaceful, parliamentary processes. And Trevelyan found a lesson for the fascists of his own time in the tolerance practiced by the Whigs: "Kindly old England has always in the long run revolted against 'fascist' experiments at the permanent suppression of 'the other side.'" ⁷³

At the pinnacle of his career, at the age of fifty-nine, Trevelyan received a distinct honor that gave official approval to the Whig interpretation of history. It had become the English view of history, with the seal of the Crown, for G. M. Trevelyan was invited to write King George V's speech to both houses of parliament at Westminster Hall on May 9, 1935, in honor of the monarch's Silver Jubilee on the throne. The speech bore the clear mark of Trevelyan's pen. "Beneath these rafters of medieval oak, the silent witnesses of historic tragedies and pageants," the aging king reminded the lords and M.P.'s, "we celebrate the present under the spell of the past. It is to me a source of pride and thankfulness that the perfect harmony of our Parliamentary system with our Constitutional Monarchy has survived the shocks that have in recent years destroyed other empires and other liberties." And rounded phrases summed up the Whig philosophy: "The complex forms and balanced spirit of our Constitution were not the discovery of a single era, still less of a single Party or of a single person. They are the slow accretion of centuries, the outcome of patience, tradition and experience constantly finding channels old and new for the impulse towards liberty, justice and social improvement inherent in our people down the ages." ⁷⁴

The speech constitutes something like a symbolic acknowledgment that the Whig interpretation had become the accepted, popular consciousness of British history. It is especially appropriate that the speech should implicitly

⁷³ GMT, *Protestant Succession*, 191, 161, 320-21, 96.

⁷⁴ Hansard, *Parliamentary Debates*, House of Lords, 5th ser., 96 (1934-35): 836-38.

make Whig-Liberals of the Fabian socialists, just as Trevelyan had already invited the Tories into the Whig enterprise. All England, it seems, was Whig. And the Whig interpretation was particularly suited to imply this kind of loose consensus among varieties of political persuasion; for the virtues it designates, while positive in their own right, are matters of political procedure and intellectual civility rather than of precise confining programs and are therefore compatible with many ideologies so long as these do not of their nature preclude tolerance, diversity, and cooperation.

In a letter to his brother, now Sir Charles Philips Trevelyan, a left-wing Labourite, George made clear that despite their political differences any revisionist socialist like Charles could be included within the consensus: "I do not expect any future coolness between us, partly because we now realize our own and each other's latent tendencies apart, better than we did in 1914. And partly because you, tho' you want things I don't want, want them by constitutional means and coupled with the preservation of freedom." He then contrasted his own philosophy with his brother's: "I care *much* more about individual freedom as the precondition of good civilization than about anything else in politics and society. We two have certainly had the most extraordinary advantages, and I the most extraordinary good luck in life. If I have availed myself of my chances well, it is partly because I have not gone in for the rough and tumble of the world's debate. I think I was right, as my gifts were literary not administrative or political. But no one knows better than I do that I am no hero."⁷⁵

And while G. M. Trevelyan was not especially heroic in those days of Baldwin and Chamberlain, he did take action when he thought his personal involvement could be helpful. For example, in 1933 along with Lord Rutherford, the physicist, Trevelyan helped form a society to assist the Jewish and anti-Nazi university teachers being expelled from Hitler's Germany; about six hundred academics were secured worthy positions in Britain.⁷⁶ Trevelyan also was a constant and enthusiastic conservationist both as chairman of the Estates Committee of the National Trust and as president from 1930 to 1950 of the Youth Hostels Association.⁷⁷

During the 1930s Trevelyan also completed two works of love, biographies of his father and of their distinguished Northumbrian neighbor, Sir Edward Grey. In these two books Trevelyan recorded a good deal about his own views in the interwar era. In describing the attitude of his father toward World War I, Trevelyan was echoing his own pessimism: "He regarded the war as the most unmitigated calamity and fully realised that it made an end of the civilisation to which he belonged." Both father and son felt out of place in the postwar world: "The grey dawn of peace showed that the land-marks he knew had vanished in the night, and he was too old to join in the search for their

⁷⁵ GMT to CPT, Mar. 17, 1934, CPT Papers, University of Newcastle-upon-Tyne, packet 242.

⁷⁶ Lord Beveridge to the editor, the *Times* (London), July 24, 1962.

⁷⁷ GMT, *Must England's Beauty Perish?* (London, 1929); Jack Catchpool, "G. M. Trevelyan, O.M., C.B.E.," *Youth Hosteller*, Sept. 1962, pp. 10-12.

successors. He lived in almost complete seclusion with my mother at Wallington and Welcombe, reading and dreaming of the past and awaiting in patience the too long lingering end.”⁷⁸ In *Grey of Fallodon* G. M. Trevelyan held up the Liberal aristocrat and hesitating foreign secretary of 1914 to the British of 1937 as a symbol of the Whig tradition: “For many troubled years, and at one terrible crisis, he had represented England at her best—her reasonableness, her justice, her desire for peace and friendship between all, and with that her determination not to be frightened into a submission or dazed into a tardiness that would allow one Power time to enslave the world.”⁷⁹

Shortly before the English people would be tested as they had not been since Napoleon, Trevelyan published “a masterpiece in miniature”⁸⁰—*The English Revolution, 1688–1689*. The little book is remarkable for its stimulating analysis of the Revolution and its distillation of his Whiggish views. Rather than being called “Glorious,” wrote Trevelyan, the Revolution of 1688 should bear the title “The Sensible Revolution.” He stressed the moderation: “The true ‘glory’ of the Revolution lies not in the minimum of violence which was necessary for its success, but in the way of escape from violence which the Revolution Settlement found for future generations of Englishmen.” But at a time when the fascist threat was growing, Trevelyan seemed to emphasize the liberal side of the Whig tradition in contrast to the conservative elements he had stressed during the 1920s. And so he criticized the ultraconservative view of the Revolution that had prevailed for 150 years: “The Whig Governments before Burke, and the Tory Governments after him, all had too much reverence for the letter of the Revolution Settlement. It became a flag of ultra conservatism, first Whig, then Tory. To Walpole, Blackstone, Burke, Eldon and the anti-Jacobin Tories of the early nineteenth century, the year 1689 seemed the last year of creation, when God looked upon England and saw that it was good.” Trevelyan was of two minds on the bearing of the Revolution Settlement on his own time. He was aware of the disadvantages of the English parliamentary system: “The compromise system of the Restoration, though very useful in its day, has led to weakness abroad and constant strife at home.” Obviously with Hitler and Mussolini in mind, he warned his countrymen: “The system of government by discussion has its disadvantages, under which in new forms we are labouring today, in face of absolutist governments of a new and more formidable type than those of Europe of the *ancien regime*.” Yet mindful that under the “Sensible Revolution” Britain obtained not only political liberty but also national power, greater than that of Louis XIV’s absolutist regime, Trevelyan said in Churchillian tones: “But unless strength upholds the free, freedom cannot live. And the Revolution Settlement gave us strength as well as freedom.”⁸¹

⁷⁸ GMT, *Sir GOT*, 145–46.

⁷⁹ GMT, *Grey of Fallodon* (London, 1937), 365.

⁸⁰ Plumb, *Trevelyan*, 27.

⁸¹ GMT, *The English Revolution, 1688–1689* (New York, 1965), 3, 4, 8, 130–31, 8.

Despite his scholarly allusions to the fascist threat, George wrote to his brother Bob after the Munich Agreement that both Janet and he were “Chamberlainites.”⁸² Even after the official outbreak of war between Britain and Germany following the Nazi invasion of Poland, he could write: “I am more inclined to think we ought not to have guaranteed Poland than that we can break off now. But I am agnostic and have no clear opinion. On the whole I think the only chance for Europe including ourselves to escape utter ruin would be for the U.S.A. to take a part in negotiating peace—but she won’t, or at least she won’t guarantee it.” Trevelyan recalled that the last thing Edward Grey had said to him in the few weeks between the Nazi revolution and his death was “I see no hope for the world.” And GM remarked: “There is less now. One half of me suffers horribly. The other half is detached, because the ‘world’ that is threatened is not my world, which died years ago. I am a mere survivor. Life has been a great gift for which I am grateful, tho’ I would gladly give it back now.”⁸³

As he had watched Europe move toward despotism in the 1930s, Trevelyan even had begun to see the churches as allies in the struggle for individual liberty. He hoped that Christianity would survive the Nazi effort to root out “Christianity and all other forms of independent culture” from Germany, he wrote his daughter Mary; drawing on Mary’s great-granduncle Matthew Arnold, he predicted that “Hebraism and Hellenism” would draw closer together “in a common resistance to this new barbarism.”⁸⁴

With the outbreak of World War II, the Whig interpretation of English history would be back in fashion, especially in Churchill’s speeches. The individual liberty and orderly progress of which it spoke made a perfect contrast to the shrill totalitarianism of the Nazis. Trevelyan realized the current significance: “The liberalism of our English interpretation of our *own* history is accepted—would an English liberal history of modern Europe down to 1935 be equally welcome? I don’t know.”⁸⁵ In the face of the Nazis even Herbert Butterfield, the chief critic of the Whig interpretation, would write about the “wonderful effect” that view of history had achieved in shaping the course of English history.⁸⁶

During the spring of 1940, Trevelyan, the biographer of Garibaldi, in a broadcast over Radio London beamed into Italy, urged Italian neutrality.⁸⁷ In private he could not be an optimist. After Dunkirk and at the beginning of the Battle of Britain, he sorrowed to his brother: “I fear we live in days when any-one of our generation who dies is lucky. I never had any real hope for the world after this war broke out, but it is all going even worse than I feared. But

⁸² GMT to RCT, Oct. 18, 1938, RCT Papers, box 14, no. 141.

⁸³ GMT to RCT, Oct. 4, 1939, *ibid.*, no. 149.

⁸⁴ GMT to Mary Moorman, Apr. 28, 1935, quoted in Chadwick, *Freedom*, 33. For discussion of Hebraism and Hellenism, see Matthew Arnold, *Culture and Anarchy* (London, 1869), *passim*.

⁸⁵ GMT to RCT, Feb. 21, 1940, RCT Papers, box 14, no. 156.

⁸⁶ Herbert Butterfield, *The Englishman and His History* (Cambridge, 1944), 7.

⁸⁷ Trevelyan spoke with “such obvious sincerity and fervour” that, as he left, the English announcer who was at the controls exclaimed, “What a nice old boy.” Uberto Limentani, “Radio Londra durante la guerra,” in *Inghilterra e Italia nel 1900: Atti del Convegno di Bagni di Lucca, Ottobre 1972* (Florence, 1973), 204.

I can see no course for us now but to fight on for it is not an enemy from whom we can get terms or agreement." And he now lamented Britain's failure to arm properly: "The combination of defying great enemy powers without arming as they were armed has been a hideous and fatal folly. But we must now make the best of it."⁸⁸

During the autumn of 1940, Trevelyan's Regius Professorship was drawing to an end, under the age limit of sixty-five. The master of Trinity College, Cambridge, Sir Joseph Thomson, had died in August; the mastership of Henry VIII's foundation is the one headship of a Cambridge college that the Crown appoints. And as noted, Churchill was delighted to advise the king to appoint a living symbol of the college's great tradition, George Macaulay Trevelyan. "The fellows are very anxious I should accept, the more so as they do not know whom Winston would appoint if I refused . . .," Trevelyan reported to Bob. "That the crash of civilization should have landed me in the beautiful old lodge with its beautiful old-world traditions of Montagu Butler and Whewell and Bentley is a tragi-comic irony! But we shall not move in there till January and meanwhile it may be destroyed by a bomb!"⁸⁹

During the war Trevelyan published his most popular and timely work, *English Social History*. Most of the book, beginning with the fourteenth century and concluding with the death of Queen Victoria, had been written in peacetime, but when the war interrupted his task, he abandoned the idea of covering the centuries before the Age of Wycliffe and finished the manuscript during the early months of the war. Because of the paper shortage, his survey of six centuries of social history was published first at New York in 1942 and not until 1944 by Longman in London. Plumb describes the book as an "elegy" to a lost way of life, with a "sunset glow . . . softening the edges."⁹⁰ If Trevelyan's besieged countrymen were feeling nostalgia for a peaceful, rural past, his *Social History* must have appealed to them. It was a poetic account—the rough places smoothed in a flowing and often elegant style. Trevelyan described the swampy Fenland of Elizabethan England as "still a world by itself" where "the fenmen continued to dwell round its shores and on its innumerable oozy islands—living an amphibious life, and varying their traditional occupations with the changing seasons of the year." "In the midst of these scenes of wild nature," he observed, "Ely Cathedral had for centuries floated like an ark upon the waters, its two towers and two long shining roofs far seen on distant horizons." And in describing the southern English countryside of his favorite period, the Augustan Age, he made the landscape match the moderation of the era's politics: "Everywhere that perfectly beautiful equilibrium between man and nature, which marked the Eighteenth Century landscape, was in process of being established. While hedgerow and orchard were gaining on the wild, the multiplication or improvement of cottages,

⁸⁸ GMT to RCT, July 12, 1940, RCT Papers, box 14, no. 159.

⁸⁹ GMT to RCT, Sept. 27, 1940, *ibid.*, no. 158.

⁹⁰ Plumb, *Trevelyan*, 32–33.

farm-buildings and Halls was going on, either in old traditional styles, or in that dignified but simple manner which we know as 'Queen Anne.'"⁹¹

Moderation and equilibrium continued to determine Trevelyan's political attitudes throughout the war and afterwards. Though a friend and school-mate of Churchill, he could be critical of the prime minister's rashness. Greece, he told his brother Bob, "I think has been badly muddled by Churchill whose obstinacy is sometimes a blessing but sometimes the reverse."⁹² Another letter to Bob spoke favorably of the Labour victory in the general election of 1945: "I like the result of the election, I think, though of course things may turn out badly yet, as they often do. But I don't see how a purely Conservative govt. could in the present conditions have turned out well." While he must have felt closest to the Liberal party, he wrote: "And tho' I agree with the Liberals well enough, I should greatly fear that if they had held the balance we should have had long delay in getting any govt.—and then have got a weak one,—very fatal in the present state of the world. The Labour leaders have now had some very realistic experience in government which they so sorely lacked before 1940. I hope they will nationalize the mines."⁹³ Once again experienced Labourites and moderate socialists would be ranked within the consensus portrayed in the new Whig interpretation.

As Trevelyan entered his seventies after World War II, he dealt, like many aging historians, with fundamental historiographical questions. His last important historical work, published in 1949, was *An Autobiography and Other Essays*. In addition to a fifty-one page autobiographical essay, the book includes such lectures and essays as "History and the Reader," "Bias in History," and "Stray Thoughts on History." In all of these Trevelyan reveals a subtle and sophisticated view of history, certainly not the simplistic Whig view attacked by Herbert Butterfield in *The Whig Interpretation of History*, which, in Trevelyan's view, must have been directed against him because he was "the last Whig historian in the world," he mused to the young tutor of Peterhouse.⁹⁴

In his presidential address to the Historical Association in 1947, Trevelyan was anything but the "unhistorical," present-minded Whig when he urged historians to "get inside the minds of the people of the Middle Ages and of Tudor times and of the Eighteenth Century, and see their problems as they saw them, not as we see them now." Trevelyan argued that a historian's bias could sometimes help him to sympathize with the passions of people in the past, and he used his Garibaldi trilogy as an example: "I once wrote three volumes on Garibaldi. They are reeking with bias. Without bias I should

⁹¹ GMT, *English Social History, A Survey of Six Centuries: Chaucer to Queen Victoria* (New York, 1942), 147, 149, 304.

⁹² GMT to RCT, Dec. 25, 1944, RCT Papers, box 14, no. 190.

⁹³ GMT to RCT, July 31, 1945, *ibid.*, no. 193.

⁹⁴ Butterfield, quoted in Chadwick, *Freedom*, 37. According to Chadwick, *ibid.*, 37–38, there is a discrepancy between "what the book said and what it was supposed to have said." Butterfield told Chadwick that in the *Whig Interpretation* he did not have Trevelyan in mind, "but primarily Acton."

never have written them at all. For I was moved to write them by poetical sympathy with the passions of the Italian patriots of that period, which I retrospectively shared. Such merit as the work has, largely derives from that. And some of its demerits also derive from the same cause.”⁹⁵ Trevelyan even assessed this kind of bias as “imaginative sympathy”—the phrase and method Butterfield had proposed for interpreting the past.⁹⁶ Without mentioning him by name, Trevelyan appeared to be answering Butterfield’s contention that the Whig historian’s “greatest limitation” would be a “defect of imaginative sympathy.”⁹⁷ In criticizing the “fervour of the whig historian,” Butterfield had asserted that “the true historical fervour is the love of the past for the sake of the past.”⁹⁸ Trevelyan accurately described this view as a form of bias.

In “Stray Thoughts on History,” an essay of 1948 discussing the philosophy of history, Trevelyan even sounded like a scientific or objective historian. Every event, “for good or for bad,” he argued, “wears for ever the inviolable sacredness of the accomplished fact. The statue has taken its shape and can never go back into the quarry.” Judgments about the past are based “on the assumption that if things had gone differently they would have been better—or worse.” But this is something that can never be discovered, and the “opinions” of historians should be held with “a modest diffidence.”⁹⁹

Yet we may suspect that Trevelyan expressed his heart’s true sentiments when he disparaged the “impartial historian,” declaring Sir Walter Scott “a great historian.” “He was all the greater,” proclaimed Trevelyan, “because he had feelings to control. The impartial historian who has no feelings to control, rides a sorry nag to market; Sir Walter bestrode a war-horse that never ran away with him.”¹⁰⁰

In his autobiography Trevelyan maintained that he had been “not an original but a traditional kind of historian”—an assessment with which many historians would agree. “The best that can be said of me,” he continued, “is that I tried to keep up to date a family tradition as to the relation of history to literature, in a period when the current was running strongly in the other direction towards history exclusively ‘scientific’, a period therefore when my old-fashioned ideas and practice have had, perhaps, a certain value as counterpoise.”¹⁰¹ Though very much in the Macaulay-Whig tradition, Trevelyan put his own mark on it by transforming a partisan view into a concept of a consensus within the modern British political system.

Despite his modesty and his subtleties, Trevelyan continues to be labeled as a journalistic historian whose scholarship is in doubt, largely perhaps because he did not “belong to the professional guild of teaching historians.” The

⁹⁵ GMT, *Autobiography*, 77.

⁹⁶ *Ibid.*, 78; Butterfield, *Whig Interpretation*, 95.

⁹⁷ Butterfield, *Whig Interpretation*, 95.

⁹⁸ *Ibid.*, 96.

⁹⁹ GMT, *Autobiography*, 90–91.

¹⁰⁰ *Ibid.*, 204.

¹⁰¹ *Ibid.*, 1.

criticisms tend to be self-perpetuating, and one particularly unfortunate example should be noted here. In a recent BBC television documentary entitled *About Trevelyan*, part of the Open University's Introduction to History series, Professor Arthur Marwick contrasted his own scientific view, based on empirical evidence, with Trevelyan's focus on "questions of passion and value" and "different personal and political views." Trevelyan's *English Social History*, according to Marwick, was "riddled through and through with general pronouncements which no doubt appealed to Trevelyan's readers, but which had no basis in historical evidence." In effect, Trevelyan was classed with pop historians by Marwick and other commentators in the twenty-five-minute program, and his Whig style of history was denounced for its "cavalier treatment." What Trevelyan needed, Marwick commented, "was the sceptical voice of a student audience, and, of course, the criticism of teaching colleagues."¹⁰²

Though Marwick recognized it as "in many ways unfair to pick out isolated quotations," he nevertheless selected two extracts from Trevelyan's *English Social History* as examples of the "ludicrous generalities which, note, are never abrasive, but always designed to make his readers feel nice and warm inside."¹⁰³ Strangely enough for one dedicated to scientific history, Marwick failed to reveal how extremely "unfair" he was in quoting Trevelyan completely out of context and giving quite the opposite impression from that conveyed by the full passages.¹⁰⁴ Apparently Marwick's personal philosophy

¹⁰² Transcript of *About Trevelyan* in *Humanities: A Foundation Course*, Introduction to History, BBC Open University production, Nov. 14, 1971, pp. 3, 17, 3, 7, 4. See the Open University, Harper and Row, film no. 2800084. In addition to Marwick, professor of history at the Open University, the other participants in the telecast were R. Barrie Dobson, reader in history, University of York; Joel Hurstfield, Astor Professor of English History, University College London; and Keith Robbins, professor of history, University College of North Wales.

¹⁰³ *Ibid.*, 4.

¹⁰⁴ In pointing out how Trevelyan's *English Social History* was "riddled through and through with general pronouncements . . . which had no basis in historical evidence," Marwick remarked: "Writing of Shakespeare's England Trevelyan refers to the Spanish Inquisition and the shambles which religion produced in the Netherlands and in France. He continues: 'Looking across the channel and seeing these things, the English rejoiced that they were Islanders and that wise Elizabeth was their queen.' Most notorious of all is the reference to the happy English and the unhappy French and their revolution: 'If the French Noblesse had been capable of playing cricket with their peasants, their chateaux would never have been burnt'" (*About Trevelyan*, 4). Marwick extracted the first passage from the following paragraph in Trevelyan's *English Social History* (pp. 140-41): "There is, of course, another side to all this, as there is to every picture of human well-being and well-doing. The cruel habits of centuries past were not easily or quickly to be shed. The overseas activity of the Elizabethans paid no regard to the rights of the negroes whom they transported into slavery, or the Irish whom they robbed and slaughtered: some even of the noblest English, like John Hawkins on the Gold Coast and Edmund Spenser in Ireland, failed to see what dragons' teeth they were helping to sow. At home, the woman hunted by her neighbors as a witch, the Jesuit missionary mounting the scaffold to be cut to pieces alive, the Unitarian burning at the stake, the Puritan dissenter hanged or 'laden with irons in dangerous and loathsome gaols,' had little joy of the great era. But in Elizabeth's England such victims were not numerous, as elsewhere in Europe. We escaped the pit of calamity into which other nations were being thrust—the Spanish Inquisition and the vast scale of martyrdom and massacre that turned the Netherlands and France into a shambles in the name of religion. Looking across the channel and seeing these things, the English rejoiced that they were islanders and that wise Elizabeth was their Queen." Marwick's second "notorious" reference comes from the following passage (*English Social History*, pp. 407-08): "In Stuart times cricket had grown up obscurely and locally, in Hampshire and Kent, as a game of the common people. The original method of scoring, by 'notches' on a stick, argues illiteracy. But in the early Eighteenth Century cricket enlarged both its geographic and its social boundaries. In 1743 it was observed that 'noblemen, gentlemen and clergy' were 'making butchers,

so clashed with Trevelyan's that he abandoned canons of objectivity and fairness. In his book *The Nature of History*, Marwick even attributed the "widespread feeling" that social history is "somehow an intellectually inferior version of the real thing" to Trevelyan's "travesty of an *English Social History*, written during the First [sic] World War when patriotic sentiment was at its height."¹⁰⁵ *Mirabile dictu!* Trevelyan's historical works are indeed full of comments that may be misconstrued by the easy reader, but his sense of irony and his poetic insights reveal that committed history, despite its bias, can also be good history.

Upon retirement as master of Trinity in 1951, Trevelyan revealed that he remained a romantic Liberal until the end, in contrast to his brother Charles:

The ultimate prospect of a completely socialist state . . . attracts you and repels me. . . . The things that I care about *most* (though I also care a great deal about the welfare of the mass of people) are literature, art, imagination and free intellect; they seem to me to be conditioned by a certain amount of leisure for some people and independence of mass orders such as will be issued on all subjects by a completely socialist state. . . . A certain amount of inequality of opportunity and leisure seems to me essential to the things I care about, but it is going fast and will in fifty years time be gone. So I don't look forward to your millennium, tho' I fear it is an inevitable consequence of machinery and the industrial revolution.¹⁰⁶

In his final words to appear in print, Trevelyan admitted that he had grown more cynical over the years. When as a Harrow boy in 1889 he first read Gibbon's remark in the third chapter of *The Decline and Fall* that history "is little more than the register of the crimes, follies and misfortunes of mankind," he thought the comment was untrue. "I think so less now, I fear," Trevelyan remarked, "but we must add that history is also the register of the splendour of man, and of his occasional good fortune, of which our island has had more than its share."¹⁰⁷

Trevelyan certainly remained old-fashioned so far as his private correspondence was concerned. "I never wrote a private letter worth printing," he claimed¹⁰⁸—a comment that this article hopes to refute. His Victorian sense of propriety masked a practical fear of modern biographers like Lytton Strachey. When Sir Sydney Roberts, who had served as vice-chancellor of Cambridge and secretary of the university press, suggested that the full text of Macaulay's diary, preserved in the Trinity library, ought to be

cobblers or tinkers their companions' in the game. Three years later, when Kent scored 111 notches against All England's 110, Lord John Sackville was a member of the winning team of which the gardener at Knole was captain. Village cricket spread fast through the land. In those days, before it became scientific, cricket was the best game in the world to watch, with its rapid sequence of amusing incidents, each ball a potential crisis! Squire, farmer, blacksmith and labourer, with their women and children come to see the fun, were at ease together and happy all the summer afternoon. If the French *noblesse* had been capable of playing cricket with their peasants, their chateaux would never have been burnt."

¹⁰⁵ Arthur Marwick, *The Nature of History* (London, 1970), 59.

¹⁰⁶ GMT to CPT, Oct. 29, 1951, CPT Papers, packet 244.

¹⁰⁷ *The Speech of George Macaulay Trevelyan made on the 12th November 1955 at the Dinner in Christ's College, Cambridge, Given by Mr. Mark and Lady Elizabeth Longman to Celebrate the Publication of Studies in Social History: A Tribute to Trevelyan* (London, 1956), 6-7.

¹⁰⁸ GMT, *Autobiography*, 1.

published, he was surprised by the vehemence of Trevelyan's opposition. "Over my dead body," Trevelyan replied, "I'm not going to have those Bloomsbury people laughing at my great-uncle."¹⁰⁹ Trevelyan also opposed any full-scale biography of himself and took no chance with his personal papers in his possession: he destroyed them all shortly before his death, which came on July 21, 1962. "May my malison alight on anyone who attempts to publish any of the scrawls that were once my letters," he wrote; "I trust they have all long ago passed through the waste-paper basket."¹¹⁰ Perhaps his shade will lift that curse from one who found a few scattered and nearly flawless gems.

The polish of the works that G. M. Trevelyan bequeathed his profession tells of great pains, and Trevelyan relates "transcribing each paragraph four times on the average before the typing stage."¹¹¹ Each sentence is carefully worked; many paragraphs are poetic and intuitive. Trevelyan concluded a chapter on the Norman Conquest, for example, with an ironic insight into the shaping of the English language and nation during the following three centuries: "It is symbolic of the fate of the English race itself after Hastings, fallen to rise nobler, trodden under foot only to be trodden into shape." And Trevelyan evaluated the progress of reform campaigns in the 1820s and 1830s with amazing accuracy: "The Duke [of Wellington], it is reported, once complained of Peel that he never foresaw the end of the campaigns that he began. Though even more true of the Duke himself as a politician, the criticism of Peel is not unjust, provided we add, in Cromwell's words: 'None goes so far as he who knows not whither he is going.' It is characteristic of the England of that period of rapid transition, that her greatest statesmen could never see four years ahead."¹¹² As G. Kitson Clark has remarked, Trevelyan was able to do three things "which the analysts among historians are sometimes unwilling to do. He could paint a scene, he could tell a story and he could describe a man. Each is an essential service, unless history is to be resolved into a series of unrelated facts and ideas and is to disregard the unity of life."¹¹³

A "peculiar felicity of expression" was the way John Adams described the style of the Whiggish Thomas Jefferson, and there is an equivalent felicity in the thought and language of G. M. Trevelyan. The judgment that Carl Becker has made of the Jeffersonian style can be applied cautiously to Trevelyan's—that the clarity and gracefulness reflect a philosophy often just a little easy about existence, just lacking a sense of ambiguity, mystery, and rough edges.¹¹⁴ Yet Trevelyan does frequently capture the waywardness of history and its ironic complications.

¹⁰⁹ Quoted in S. C. Roberts, *Adventures with Authors* (London, 1966), 121.

¹¹⁰ GMT, *Autobiography*, 1.

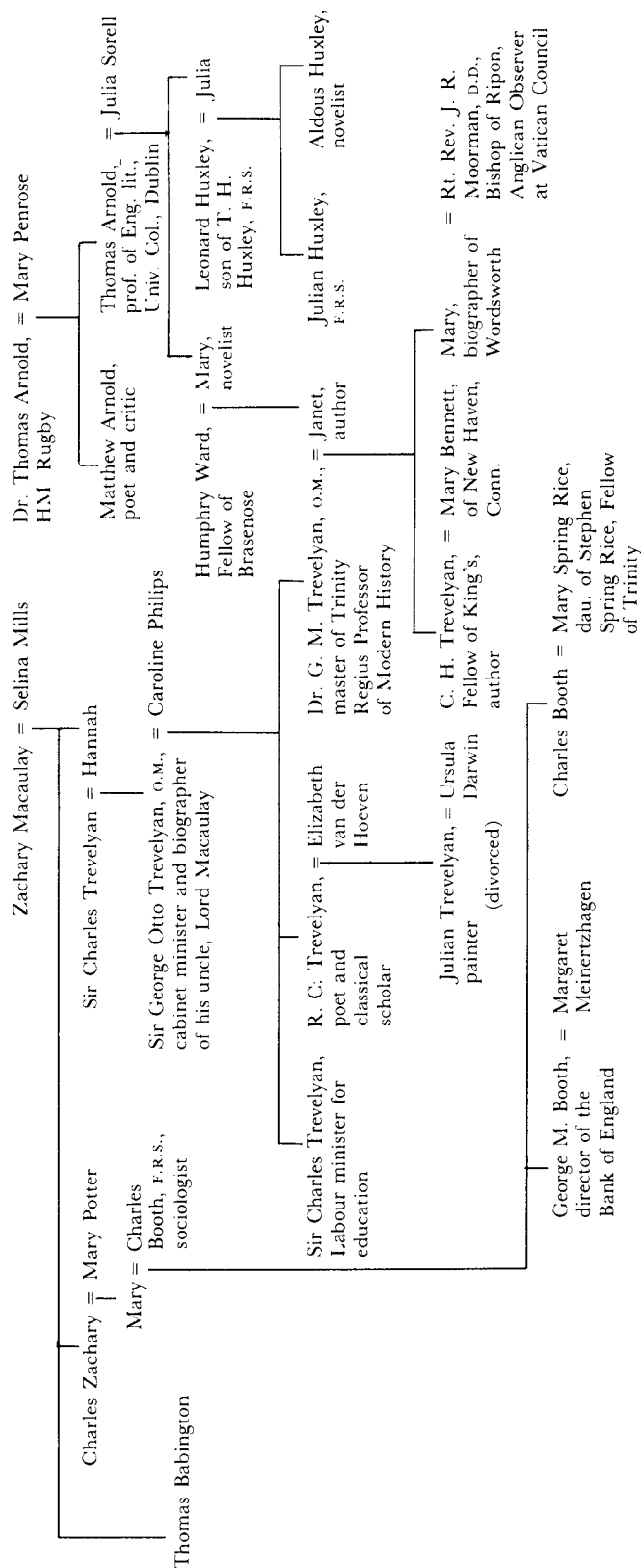
¹¹¹ *Ibid.*

¹¹² GMT, *History of England*, 132, 625.

¹¹³ Kitson Clark, "Trevelyan as an Historian," 4.

¹¹⁴ Adams quoted in Carl Becker, *The Declaration of Independence* (New York, 1922), 194; see also chapter 5, "The Literary Qualities of the Declaration," *ibid.*, 194–223, for comments on the Jeffersonian style.

THE TREVELYAN CONNECTION



Source: N. G. Annan, "The Intellectual Aristocracy," in J. H. Plumb, ed., *Studies in Social History: A Tribute to G. M. Trevelyan* (London, 1955), 259. Revised and updated by author.

If Macaulay is “the Rubens of historiography,”¹¹⁵ then G. M. Trevelyan is its Gainsborough. Like the Flemish master, Macaulay is known for his vivid and massive portraiture; in the style of his fellow countryman Gainsborough, Trevelyan is noted for his graceful and dignified portraits. Macaulay was a grand impressario whose monumental style was brilliantly colored, steeped in classical literature, and inspired by the Renaissance; his grandnephew was a more self-conscious artist whose restrained style was more English with its soft pastels and careful brushwork. As Whig history moved from the lobby to the lecture hall, becoming less polemical and more respectable in the rarefied air of the academy, the rhetorical exuberance of Macaulay gave way to the measured poetry of Trevelyan.

¹¹⁵ G. P. Gooch, *History and Historians in the Nineteenth Century* (Boston, 1959), 280.

Reviews of Books

GENERAL

MICHAEL BARKUN. *Disaster and the Millennium*. New Haven: Yale University Press. 1974. Pp. x, 246. \$10.00.

Millenarian social movements have long been of interest to historians, sociologists, political scientists, and anthropologists, but each discipline has tended to use its own cover terms and to select particular kinds of cases. Historians have focused on millennial Christianity; anthropologists on nativist movements like the Ghost Dance and on cargo cults like the Vailala Madness; sociologists and political scientists on contemporary revolutionary totalitarianisms. Barkun, a political scientist at Syracuse, in *Disaster and the Millennium*, considers the whole range of phenomena that have been investigated by these several disciplines and attempts, not so much to develop a new typology (a none-too-profitable exercise anyway), but rather to generate a statement of the necessary and sufficient conditions under which movements of the millenarian kind occur. His method is inductive: he reviews the cross-cultural and historical literature on dozens of well-reported movements and infers from the evidence the causal nexus common to all. The book thus describes a search for, and proposes, a historical law of millenarian events.

Barkun's thesis is that millenarian movements occur in the aftermath of disaster (which may be social as well as physical, of course), that they tend strongly to happen in rural rather than urban communities (rural areas being more vulnerable than cities to social disaster), and that they must have available for elaboration an appropriate and already disseminated millenarian doctrine (such as Christian millenarianism, Jewish messianism, and Muslim Mahdism). Only in recent history has millenarianism become acclimated to the city; and here, he suggests, it is because the urban-based totalitarian state, like Nazi Germany, established by revolution, uses terror as a device for maintain-

ing a continuous condition of disaster in order to perpetuate the millennialist fervor.

The effort to develop more adequate theory, in an area of study that all too easily degenerates into parochial schemes of classification, can only be praised. I am also glad to be able to say that, in matters of substance of which I have personal knowledge, Barkun is careful and accurate in the use of ethnographic and historical detail.

ANTHONY F. C. WALLACE
University of Pennsylvania

JOHN T. NOONAN, JR. *Power to Dissolve: Lawyers and Marriages in the Courts of the Roman Curia*. Cambridge, Mass.: Belknap Press of Harvard University Press. 1972. Pp. xix, 489. \$15.00.

Various strains—scriptural directives (I Cor. 7), papal enactments (for example, of Calixtus I, 217–22 A.D.), and conciliar decisions (of Elvira, 300 A.D.)—have converged through many centuries to forge the marriage law, which, since 1917, has been synthesized in canons 1012–1143 of the Code of Canon Law. The development of this vast construct is portrayed in A. Esmein, *Le mariage en droit canonique* (1929–35), and in G. H. Joyce, *Christian Marriage* (1948), but it has remained for Dr. Noonan's book to supply in-depth analysis of the adjudication in six matrimonial cases—Toul, January 15, 1653; West Lisbon, July 8, 1724; Cordoba, March 26, 1763; Rome, November 23, 1923; New York, February 8, 1915; Baltimore, June 30, 1910—that reached Roman church tribunals—the Rota, Propaganda Fide, Congregation of the Council, and Apostolic Signature—during the seventeenth to the twentieth centuries.

The achievement can be appraised in two fashions. If the subtitle, *Lawyers and Marriages*, specifies the topic, then Noonan has succeeded well. His six cases, with background and color, each cast in its cultural framework, each etched competently, are conducted to final judgment, though often meanderingly. One may quarrel, perhaps, with an inter-

pretation (pp. 87, 337, 403) or question a speculation (pp. 184–86, 317), yet the whole creates a vivid experience of curial litigation. Whether the cases are typical depends upon how well versed one is with the fifty-four annual volumes of the modern Rota.

If, however, the six instances are meant to illustrate “power to dissolve,” they are poorly chosen. Canons 1118–27 make provision for true matrimonial dissolution, that is, for the termination of genuine unions. Noonan’s final chapter shows him fully cognizant of these canonical procedures. Not one of the six cases involves the use of such power. Sought in each of the trials is not dissolution of a valid marriage but a decision that the union under review has been null from the very beginning because of factors such as coercion, conditional contract, mental incapacity, intention to exclude offspring, intention against indissolubility, or invalidity of an episcopal dispensation from a diriment impediment. Factually, in four of the six cases—Toul, Cordoba, New York, and Baltimore—the court found for the validity of the marriage. In the other two, negative judgment was given upon the reality of the union: in West Lisbon, because the intention, contractually expressed, to enter a dissoluble virginal marriage, made the union null from the start; in Rome, because the male’s positive exclusion of procreation vitiated from the beginning his marital consent. None of the cases has to do with the power to dissolve. Noonan’s remark that the distinction between annulment and dissolution “is functionally difficult to distinguish” (p. xvi) only obfuscates the situation.

Curial handling of marriage trials has raised questions and found answers “often shrewd, imaginative, instructive and, if sometimes harsh, unrealistic or oversubtle, were sometimes soberly convincing” (p. xvii). But the intricacies and delays (formidable in the New York case) here depicted make it possible to understand why the recent studies of Lawrence G. Wrenn, editor, *Divorce and Remarriage in the Catholic Church* (1973) and W. Bassett and P. Huizing, editors, *The Future of Christian Marriage* (1973), and the October 1974 meeting of the Canon Law Society of America (*National Catholic Reporter*, Nov. 1, 1974) have called for wide changes in Catholic marriage legislation.

HENRY G. J. BECK
Darlington Seminary

WILLIAM H. MCNEILL. *Venice: The Hinge of Europe, 1081–1797*. Chicago: University of Chicago Press. 1974. Pp. xvii, 334. \$10.75.

William H. McNeill’s broad interpretive essay describes some major aspects of “cultural interaction

amongst the diverse peoples of southern and eastern Europe” (p. xv) in the medieval and early modern periods. Tracing relations among them through seven centuries, McNeill considers cultural and institutional changes brought about by the emergence of new political forces and by evolving technologies of government, warfare, and economic activity; but he also keeps a steady focus on certain constants of the areas connected by the eastern Mediterranean and Black seas. One of these is Orthodox Christianity centered in Constantinople; but the most important is the Venetian Republic. Although sometimes Venice’s connection with McNeill’s discussion appears tenuous, overall the various Venetian roles as recipient, transmitter, and originator of cultural currents between East and West constitute the most important leitmotiv in the book.

There are two main subjects in the book, each dominating half of the chronological sweep and half of the body of the book. The first, and less successful, traces the growth and eventual triumph of Latin influence in the Byzantine Empire by virtue of the superiority of Frankish knightly combat methods and the political and economic organization of the Italian city-states, especially Venice. McNeill’s familiarity with the literature does not seem as strong here as it is in the later section, and he ventures some interpretive suggestions that come across as highly speculative and rather unconvincing. Moreover the lines of “cultural interpenetration” are not clearly drawn. Latin military tactics and politico-economic organization surely did lead, at least temporarily, to Latin domination over the Byzantines; but the cultural exchange that resulted from the encounter is not laid out forthrightly.

The section dealing with the sixteenth through eighteenth centuries is another matter. Here Venice is no longer triumphantly leading the Latin domination of the Byzantine East but steadily giving way to the power of the Ottoman Empire. McNeill is most successful in portraying Venice as a “hinge” at the time of its political and economic decline, and he does so in an original and fascinating discussion. To me the most interesting part of this section involves the complex relations between religion and government in Russia and the Ottoman Empire. In this brief review I cannot go into all the elements of the matter, but readers of McNeill’s *Europe’s Steppe Frontier* (1964) will find familiar material here. Briefly, the question involves the appearance of a rationale for governmental control over religion. In the development of the rationale, Greek alumni of the Venetian-run University of Padua were instrumental—especially in Russia—and the example of Venice’s resistance to the Counter-Reformation papacy was in-

fluent. McNeill may overstate the extent of this "collaboration" (p. 178) between Orthodox Christianity and Venice, but his enumeration of Greeks who attended Padua and then rose to influence in the Levant and Russia is suggestive—and is reinforced by his discussion of the growth of a Cretan literature bearing the traces of Italian influence during Venice's domination of Crete.

A book like this has strong and weak points. Specialists in each of the areas that it embraces may find occasion to blink. At times the reader gropes for a central conception on which to hang some of McNeill's subjects. But recognized for what it is—an essay based on wide, and occasionally deep, reading of secondary literature—the book is serious, interesting, occasionally compelling, and always suggestive. And not the least of its virtues is that it is a book that may encourage specialists in a number of fields to ask each other some valuable questions.

STANLEY CHOJNACKI
Michigan State University

SAMUEL ELIOT MORISON. *The European Discovery of America: The Southern Voyages, A.D. 1492-1616*. New York: Oxford University Press. 1974. Pp. xvii, 758. \$17.50.

The completion of *The European Discovery of America* with a volume on what are termed "the Southern Voyages" from Columbus to Schouten and Le Maire must command admiration for Rear Admiral S. E. Morison's achievement. The first volume won immediate respect for the clarity, vigor, and realism of the treatment of coastal discovery north of Cape Fear. Morison, it seemed, had read everything, been everywhere, and had known everyone who could inform him on his subject. And so it is again: the strong sense of involvement and the unrivaled capacity to describe the actions of ships and men at sea are again evident. Columbus, Magellan, and Drake dominate the volume, though very many other navigators appear and are traced with both knowledge and patience. The Columbus story is essentially the one he has imprinted on American and British readers—and many others—from 1942 onward: since *The Caribbean as Columbus Saw It* (1964), he has altered a few details on routes only and, in general, kept pace with Spanish documentary research, rather than altered his general picture. Columbus has long been a hero; now Magellan joins him and, with a little qualification, Drake. Vespucci and Sebastian Cabot are treated fairly if somewhat severely and cannot be said to have entered the pantheon. Vespucci's first voyage is repudiated (indeed there is little choice); the second voyage with Ojeda is allowed though the claim that his ship detached itself to

make the first discovery of the Brazilian coast south of Cape San Roque is ignored; the third voyage is praised for the originality and breadth of the navigator's observations, while an improved translation of part of the Bartolozzi Letter is given to illustrate these qualities (pp. 284-86), but it is not allowed further than twenty-five degrees in its southing; while the fourth voyage is passed, though in both Portuguese voyages, third and fourth, it is made clear the commander was Gonçalo Coelho, not Vespucci. All this is very much in line with recent research, though Vespucci may be thought to have been required to conform to slightly more stringent critical standards than Columbus. On the endless voyage of Magellan an almost equally endless patience is expended and many points of detail, as well as of personality, are illuminated. This is, perhaps, the outstanding achievement in the book.

On Drake there is less need for novelty, if not for argument. On the objectives of the voyage Morison agrees in general with K. R. Andrews, though without citing him. He enjoys New Albion's ambiguities, deciding on Drake's Bay for the landing place, and against the authenticity of the Plate of Brass, so that he can be assured of a warm reception by embattled West Coast partisans of other readings of the evidence. The detailed treatment of the Plate Basin exploration forms a contrast to the open seas voyages, though why not that of the Amazon as well? The book has more major voyages and figures than the first but less unity. This is partly a matter of geography, since the immense coastline from Florida to California sometimes defies a cohesive treatment, while the circumnavigations stray far from the Americas. Though so many minor voyages appear, the problems of scale are not always surmounted: in the end personal preference rather than historical import tends to prevail. It says much for the skill with which the book is brought together that these problems do not obtrude more often. Personalized experience and polemic against those who differ from him have always been an enjoyable feature of Morison's maritime works and continue to be so in this. He is more restrained in his comments on opponents in this volume, even charitable, but in the personal interventions the Ancient Mariner occasionally obtrudes himself a little irrelevantly into the story. Slips and misprints have been noticed but they are not unduly numerous for such a long and complex book—though it should perhaps be noted that the dimensions of the Cantino Map on page 272 should be multiplied by ten. A notable feature is the generous thanks to the many persons who have let him use unpublished materials—and it is clear from them that we will continue to have a flood of monographs in this general field—as well

as some stimulating suggestions for further research. The book provides a broad, floating platform from which analytical studies of the topics he introduces can be launched. This is certainly a book that no one except Morison could have written, and one that will shape the future development of the story of the earliest sea explorations of a large part of the Americas for a long time to come.

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YEHUDA ELKANA. *The Discovery of the Conservation of Energy*. With a foreword by I. BERNARD COHEN. (Harvard Monographs in the History of Science.) Cambridge, Mass.: Harvard University Press. 1974. Pp. x, 213. \$8.50.

The law of the conservation of energy influenced nineteenth-century science and effected a change in the world view. Is the formulation of such laws influenced by the thinking of the times? Professoror Elkana's thesis is that during the period when scientific theories are being formulated, the concepts used are in a state of flux. The concepts, which have their roots in metaphysics, cannot be expressed in words since they are part of the preconceptions of the scientists. The motto of the book reads, in part, "the most fruitful concepts are those to which it is impossible to attach a well-defined meaning." The connection of scientific research with the thought of the times is through this unexpressed metaphysics. Elkana has an interesting and fruitful thesis for the history of science.

Hermann von Helmholtz was a central figure in the evolution of the conservation of energy principle and was influenced by German philosophy. He was a thinker who combined physiological with physical ideas. Elkana admits that he has no direct evidence of what influenced Helmholtz's thinking. Since the book is about the flux of ideas, there is no logical sequence of development to follow. But all historians who follow the flow of ideas find that they often are more like underground streams than visible rivers that are easy to follow. It is at this point that historical training is more important to the historian of science than a scientific background. By being steeped in all aspects of the period, the historian must be able to give a convincing description of the relation between the flux of ideas and the thought of the time. When direct evidence is not available, the historian cannot, as Elkana did, place the different facts side by side and declare one's conviction that they must be related somehow. Historians like scientists work by inference and analogy not by juxtaposition.

Part of Elkana's difficulty was in considering scientific ideas in isolation, the so-called internalist

approach, prior to relating them to the thought of the times. The internalist view of the history of science cannot be integrated with anything else because that method encapsulates ideas so thoroughly that all links with the outside are severed.

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JACQUES DROZ, editor. *Histoire générale du socialisme*. Volume 2, *De 1875 à 1918*. Paris: Presses Universitaires de France. 1974. Pp. 674.

To give an audience a synoptic view of socialism in the various countries is an undertaking of great merit even if it is not entirely successful. The first volume of the symposium edited by Jacques Droz already has demonstrated the value of the underlying idea as well as some defects in its execution. The same applies to the second volume.

To the professional historian, the enormous amount of detail the book contains is very useful; he can find names and events that are otherwise mentioned only in often-obscure monographs. A price, however, had to be paid for the extensiveness of the record: in some sections, especially in the Spanish and the Russian, the flood of detail has obscured the great outlines, and in many others some questions of fundamental importance were given short shrift. For instance, although in contrast to the first volume, religious influences on the development of British socialism are now mentioned even aside from Christian Socialism proper, the treatment of this important relationship remains superficial. In the sections on Australia and New Zealand, the outstanding contribution that these countries made to social organization, the development of compulsory arbitration of wage disputes, is referred to without any attempt to show its positive and negative significance for socialism and the labor movement.

That the sections differ in quality is only natural. I was particularly impressed with the chapter on post-Commune France, written by Madeleine Rebérioux. It brings out, for example, the role that semantic ambiguities have played in the controversies among the various socialist factions in this period; it also shows convincingly the reasons why the French Marxists under the leadership of Jules Guesde made such conspicuous moves toward reformism in the 1890s before the Millerand affair; moreover it explains why in the unified party after 1905 Jean Jaurès remained the outstanding leader, although his reformist policies had been repudiated. Equally excellent is the part on the Second International by Annie Kriegel, especially the analysis of the internal tensions within the world organization. The American section,

written by Marianne Debouzy, just barely mentions the vitally important relationship of American socialism to the antislavery and Free Soil movements in the antebellum period and the equally important impact of the farmers' movements in the last three decades of the nineteenth century; both relationships deserve an analysis in depth.

The chapter on socialism during World War I, also by Madeleine Rebérioux, is on the whole inadequate in spite of some enlightening remarks. The reader does not learn at all that the German Social Democrats fought not only against the Left but also, and more bitterly, against the political Right, which had insisted on the unrestricted submarine war and demanded the annexation of Belgium and northern France. The Reichstag resolution of July 1917, which rejected any acquisition of territory by force and demanded an economic as well as a political peace, and which was passed largely on Social Democratic initiative, is not even mentioned. Perhaps the third volume, although it is supposed to start from 1918, will have a retrospective chapter to fill the worst gaps in the presentation of the war period.

Jacques Droz and some of his collaborators have tried hard to discover in which ways the socialist movements in the various countries influenced each other. It is not their fault that they succeeded only to a limited extent. Certainly every historian of socialism suspects that these crosscurrents were more important than we can document, probably because private contacts among the leaders played a decisive role. Unless unpublished memoirs or letters throw unexpected light on these connections, we can only record our regret over this lacuna in our information.

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FRIEDRICH ENGEL-JANOSI *et al.*, editors. *Denken über Geschichte: Aufsätze zur heutigen Situation des geschichtlichen Bewusstseins und der Geschichtswissenschaft*. (Wiener Beiträge zur Geschichte der Neuzeit, number 1.) Munich: R. Oldenbourg Verlag. 1974. Pp. 257. DM 38.

HEINZ ANGERMEIER. *Geschichte oder Gegenwart: Reflexionen über das Verhältnis von Zeit und Geist*. Munich: Verlag C. H. Beck. 1974. Pp. 144. DM 22.

Recent years have seen an outpouring of German-language writings on questions of historical theory and methodology. This literature forms part of the renewed international interest in historical theory. It also reflects the particular urgency in the German-speaking countries—where older historicist traditions dominated a broad segment of historical

writing until the very recent past—to find new theoretical and methodological foundations for a rapidly changing historical science.

Denken über Geschichte, edited by members of the Historical Institute of the University of Vienna with contributions by several younger West German historians and philosophers, as well as by Henri-Irénée Marrou and Adam Schaff, is an attempt at a critical assessment of historical thought and scholarship today. The volume is welcomed as an informative and, at the same time, critical introduction to the current international discussion of the theoretical foundations of history. The twelve articles cover a great deal of ground from Friedrich Engel-Janosi's discussion of modern speculative philosophers of history to Hubert Glaser's discussion of the crisis of historical instruction in the present-day German schools. Essays by Heinrich Lutz and Heinz Robert Schlette deal with the growing awareness of a crisis of modernity in contemporary historical thought. Another, by Edith Sauer, deals with the theology of history and church history since 1945. Henri Marrou traces the development of a "critical" philosophy of history in France from Raymond Aron to Paul Veyne. Adam Schaff examines the problem of subjectivity in historical knowledge. Heiner Rutte and Herta Nagl-Docekal go over relatively well known ground, although from critical perspectives, in their respective articles on Karl Popper and on the Anglo-American debate on historical explanation. Werner Post assesses historical and antihistorical perspectives in the contemporary social sciences. The concluding articles, one by Helmut Rumpel the other by Jörn Rüsen, address themselves to the problems of formulating a "theory of historical science" (*Theorie der Geschichtswissenschaft*). The contributors all agree on the central role of theory in history yet stress the inadequacy of the "objectivistic" methods of the behavioral sciences—system theory and structuralism—for historical understanding. Social science theories, it is stressed, must be integrated with hermeneutic methods (p. 234). Max Weber is mentioned repeatedly as a model in this connection. Yet remarkably little attention is paid to an examination of how working historians have in recent years actually utilized these theories in historical investigations. This is regrettable. The book deals at length with the contemporary philosophical discussion in Germany and elsewhere but ignores the important reorientation that has taken place in historical studies in the most recent years.

Heinz Angermeier in *Geschichte oder Gegenwart* seeks to come to terms with two major orientations of nineteenth- and twentieth-century thought, historicism and existentialism. Both, he stresses, ex-

press modern man's drive to establish his "autonomy" in an age when all theological and philosophic points of reference external to man have collapsed. Both must fail, in Angermeier's opinion, in their search for autonomy—historicism because history by itself cannot provide a philosophy or understanding of man; existentialism, as represented by Nietzsche and Valéry, because in rejecting history it distorts man's humanity. Yet the solution suggested by Angermeier, which bridges both positions and establishes a meaningful relation between the concern with the present and the past, between *Zeit* and *Geist*, remains difficult to follow—in part because the terms *Zeit* and *Geist*, crucial to the argument of the book, elude precise definition. Moreover, the modern human sciences, which Angermeier dismisses in a few lines, with their stress on the structural context within which human behavior takes place, make it doubtful whether the "autonomism" Angermeier describes is as decisively characteristic of modern historical thought as he assumes.

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ANCIENT

BARRY CUNLIFFE. *Iron Age Communities in Britain: An Account of England, Scotland and Wales from the Seventh Century BC until the Roman Conquest*. (Archaeology of Britain.) Boston: Routledge and Kegan Paul, 1974. Pp. xviii, 389. 28 plates. \$31.75.

Someone thumbing through this initial volume in the Archaeology of Britain series might well conclude from the dozens of plates, drawings, and maps, as well as from the lack of footnotes, that the general reading public is being treated to a lavish run of coffee-table adornments authored by some of England's leading scholars. Fortunately, however, for both professionals and dedicated amateurs, this book is a serious historical synthesis of high quality that is likely to remain the basic presentation of "the state of the field" until a promised second edition appears.

In seventeen chapters of unequal length, virtually every aspect of life in Iron Age Britain, including religion, trade, social structure, military architecture, and the establishment of Roman control, is treated. In addition there are three valuable appendixes illustrating pottery types, listing selected radiocarbon dates, and enumerating the principal archaeological sites; these are complemented by an excellent bibliography. A somewhat jejune index completes the volume. The most important point to emerge from this synthesis is

Cunliffe's virtual abandonment of hypotheses of invasion and large-scale immigration and his preference for trade and peaceful interaction as agents of change and cultural influence. By dwelling on the essentially conservative nature of Britain during this period, Cunliffe makes it possible to impress the reader with the fact that more than a half millennium is being considered. This goes a long way in avoiding the telescoping of time, a fault far too common among archeologists and premodern historians.

Undoubtedly various specialized journals will devote many pages to discussions of Cunliffe's interpretation of details. Here, however, it seems more fitting to examine some problems of a more general nature. In this context it is worthwhile to emphasize that Cunliffe, by dealing with Britain (excluding Ireland), places himself in the position, at least implicitly, of permitting medieval and modern political developments to circumscribe the limits of his topic. A British Iron Age or an Iron Age Britain is a modern scholarly conceptualization that has been made attractive by deeply rooted national sentiments. That the future will probably bring us a history of Iron Age Uganda seems likely but hardly desirable. In short, much is to be gained by emphasizing for certain times and in certain localities the unity of insular and continental cultures. In addition, Cunliffe presents an abundance of evidence to illustrate the diversity of culture on the island itself and particularly between the south and east on the one hand and the north and west on the other. A second major point concerns the insufficient discussion of the difficulties raised by the substantial lack of identity between groups or societies that are known from the written sources and those known from archaeological ones. Finally, Cunliffe's treatment of numbers found in the written sources needs considerable rethinking in relation to the material evidence. Battle casualties reported at eighty thousand or even ten thousand require serious examination from the point of view of demography and logistics before being accepted. So, too, does the figure of four thousand chariots even if it comes from Caesar.

These reservations should not detract substantially from the value of an important book. Rather, they are intended to encourage more work and thought—Cunliffe makes it clear that we do not know far more than we do know—so that the benefit to professionals and students alike of the second edition will be even greater than that of the first.

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E. D. PHILLIPS. *Aspects of Greek Medicine*. New York: St. Martin's Press. 1973. Pp. 240. \$12.95.

An up-to-date historical survey of ancient Greek medicine for the general reader is much needed. Phillips's attempt in that direction is very uneven and undependable. His subject is medicine from the earliest literary allusions to Galen (ca. B.C. 700–A.D. 200).

Early history is unavoidably conjectural; the earliest medical texts, the Hippocratic Corpus, cannot be dated and are not very informative about the conditions under which they were written. Phillips does not talk about historical methodology. He says simply that his statements rest on the ancient evidence so far as possible. But he does not instruct the reader in the nature of that evidence, while he is careless in his use of it. One example must stand here for many: in the modern—not ancient—manner he attributes books of the Corpus to “schools” of different persuasions; he says repeatedly that *Regimen in Acute Diseases* is agreed to be a Cnidian work, and so he explains it. But that is the book which Galen and the moderns who study the “schools” all use as a touchstone of Coan medicine.

Still, the most useful part of the book, if used with care, is the account of what is in the *Corpus Hippocraticum*: excerpts and summaries of the medical treatises arranged by modern medical categories, with indications how to find the material in Littré, and a moderately full bibliography. This is more extensive than anything of its kind in English. Phillips's treatment of post-Hippocratic medicine is more haphazard. In the nineteenth-century manner, he classes people and periods as scientifically progressive or the opposite by modern standards, with the result that he admires the works that have been lost and depreciates those that have survived. I urge readers to study Charles Lichtenthaler's essay on that historical method, “De quelques changements dans notre conception de l'histoire de la médecine,” *Deux Conférences*, (Geneva, 1959), pp. 15–55.

Phillips dismisses, in a few pages, Galen's vast works as an incoherent system constructed from irreconcilable elements. Galen's fault, he judges, was a misplaced piety that led him to accept Hippocrates' humoral theories against superior contemporary notions—which Phillips fails to specify. He does not try to tell us what is in Galen's works and how to find it, nor can the reader trust the little information he gives. Again, one illustration: to an erroneous list of the contents of Galen's *Anatomical Procedures*, Phillips appends the remark that Galen does not seem to treat sexual anatomy. In fact book 12 of the work, easily available in English translation, is devoted to the subject.

I am sorry to say that there is still no good book

on the subject with which Phillips deals. Readers are still better off using the standard histories of medicine written early in this century.

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ROBERT DREWS. *The Greek Accounts of Eastern History*. (Publications of the Center for Hellenic Studies.) Washington: the Center; distrib. by Harvard University Press, Cambridge, Mass. 1973. Pp. 220. \$12.00.

The meat of this book is in its second and third chapters, “The Earliest Greek Accounts of Eastern History” and “The Histories of Herodotus.” Avoiding concentration on earlier theories about Herodotus' intellectual development and reflecting a more recent emphasis on the basic unity of the histories, Drews argues that Greek historiography only became established with Herodotus and that crucial to its establishment were the Persian Wars (conveniently and somewhat dramatically referred to as “The Great Event”).

In the fifth century, at least three prose writers—Dionysius, Charon, and Hecataeus—wrote *Persica*. Drews conjectures that it was the Persian Wars which inspired these works: “They were written after the Persian Wars, they described the wars and apparently did not trace Persian history beyond them” (p. 32). This conjecture Drews finds supported by the sudden emergence of Eastern historical themes in other areas of Greek literature such as tragedy, elegiac, and choral poetry. The thesis is not certain in all details (for example, one fragment of Charon—plausibly assigned to his *Persica*—tells of an event in 465–64 B.C.), and the evidence is too flimsy to support it—as Drews would be the first to admit. It is refreshing, however, that he, unlike earlier scholars, does not attempt to make the fragments prove the unprovable, and the thesis is persuasive.

The perennial problem of Herodotus' histories is the relationship (chronological and thematic) of the great logoi—Lydian, Babylonian, Egyptian—to the account of the Persian Wars themselves. Drews cuts this Gordian knot by arguing that these logoi are not intended to be full histories of the peoples concerned nor was Herodotus' interest in them “limited to substantive interconnections with the Great Event” (p. 49). His interest was in the periods when great *erga* were done and in personages who produced great *erga*. This goes to explain why certain periods when these peoples were closely tied up with events of Persian history are passed over: nothing great was produced.

This thesis has the advantage of simplicity, and I think many will find the arguments simplistic. The length and the detail, especially of the Egyptian logoi (the point of departure for C. W. Fornara's

recent *Herodotus: An Interpretive Essay*), seem disproportionate to the organization of the histories that such a thesis presumes. The book is clearly written but is occasionally repetitive and padded. The bibliographical footnotes are valuable.

JOHN J. KEANEY
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PHILIPPE GAUTHIER. *Symbola: Les étrangers et la justice dans les cités grecques*. (Annales de l'Est, Mémoire number 42.) Nancy: Université de Nancy. 1972. Pp. 402.

Rarely does a work of historical scholarship appear that combines a thorough mastery of both the primary evidence and modern literature with skillful organization, clarity of presentation, and sound judgment, and even more rarely does such a work have potential interest for the jurist, the historian of political and legal institutions, and the scholar of religious and philosophical thought. This is such a book, and it deserves wide recognition. Gauthier's basic interest is in the judicial treatment of noncitizens, *xenoi* or *étrangers*, in the Greek *poleis* and the development of legal and judicial institutions that address themselves to this question, particularly *symbola* or judicial agreements between states to provide protection for their respective citizens while in each other's jurisdiction. He has conceived his subject in a broad and generous fashion and consequently has been led to consider a wide variety of issues, some of which are really only tangential to his specific concerns. The book has benefited from this approach, and there is much to be learned here by scholars of various aspects of Greek history and civilization, as well as by those whose interests lie in comparative government and institutions and legal history.

Gauthier, who is as much at home in dealing with fragmentary inscriptions from the fifth to the second century B.C. as he is in precise textual analysis of Homer or Euripides, in penetrating the obscurities of Athenian private law or the relationships of Greek leagues in the Hellenistic Age, has digested and analyzed a great amount of material. He has been led to consider afresh such topics as *xenia* in the Archaic period, the nature of *proxenia*, the status of metics in Athens, the development of *symbolai* and their transition to *symbola*, the concept of asylum, and the nature of isopolity. Merely to indicate these topics is to convey some impression of the scope and nature of the book, which can not be properly evaluated in a brief review. By the application of detailed philological analysis, by careful attention to legal subtleties, and by sound and judicious interpretation of often ambiguous evidence against a solid basis of historical information, he has produced a significant work both of analysis and of synthesis—despite the

modest disclaimer on page 374. To mention but a few of the more important conclusions reached, Gauthier has demonstrated that there was little need for judicial agreements between communities in the Archaic Age because most *xenoi* were protected when traveling either by private, individual bonds of friendship (*xenia*) or by religious scruple; that *symbolai* (agreements between or among numerous cities that included, but were not limited to, judicial rights of *xenoi*) were not actually replaced by *symbola* (agreements usually bilateral in nature and specifically concerned with judicial protection), although the latter type became more important, because of political and economic factors especially for Athens from 350 on; and that the concept of asylum had more to do with commercial and economic than with religious considerations. Gauthier's work does not completely supersede the older, more specialized studies on which in part it is based, but it does clarify, correct, and bring together much valuable earlier work; not only that, but it is a model of logical organization that suggests the need to assemble and evaluate a variety of evidence in treating a broad conceptual topic. Doubtless the legal historian, the literary critic, or the specialist in Greek religion will take issue with specific points, but no serious scholar in these fields or in Greek political or institutional history generally can afford to ignore this masterful work. Gauthier has learned, and learned well indeed, from those who taught him or otherwise had a part in the making of this book: A. Aymard, L. Robert, E. Will, and M. I. Finley. *Symbola* reflects to their credit as well as to Gauthier's.

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ALAN WARDMAN. *Plutarch's Lives*. Berkeley and Los Angeles: University of California Press. 1974. Pp. xiii, 274. \$11.95.

In recent years, there has been a spate of books on Plutarch, ranging from the sound and readable (C. P. Jones, D. A. Russell) to the superficial (R. H. Barrow) or inept (C. Gianakaris). Professor Wardman has entered the lists with a book on Plutarch's *Lives* that is plodding in style and redundant in organization, not unlike an army training film. It is also erudite and useful in that the author distills Plutarch's ideas from an overview of the *Lives*, rather than inferring them from a single *locus classicus* (e.g., *Alexander 1* or *Cimon 2*). Unfortunately, Wardman considers Plutarch's own life unimportant and pays only scant attention to the world in which Plutarch wrote. Like a gnostic Jesus, Wardman's Plutarch is a bloodless spirit, definitely the Word but never incarnate. Despite its title, the book is not an analysis of the *Lives* but

of Plutarch's ideas abstracted from them. In dry prose and wearisome detail, Wardman describes how Plutarch fitted the great men of the Greek and Roman past to the Procrustean bed of Platonic virtue. Yet, Plutarch's obsession with the *politicus* as a paragon and the hopeless inconsistencies of his treatment of the theme display the workings of a second-rate mind, at best banal and blinkered. Plutarch's major claim to our attention has never been his simplistic notions on politics and virtue, but rather his charming capacity to clutter his writings with historical data that otherwise would have been lost. He could also tell a good story.

To be sure, Plutarch wrote historical sermons, not biographies, and his model of the ideal *politicus* was contrived and wooden, but he also had an avid curiosity about the past and a keen eye for significant trivia. It is this wealth of information on a variety of topics that has captivated posterity, yet Wardman only touches on such matters (such as Cato Minor's failure to wear shoes while serving as praetor) when they illustrate a Plutarchean attitude (a prudish disapproval of lack of decorum). Wardman discusses Plutarch's piety, his snobbery, his love for Hellas, and his Boeotian chauvinism, but he avoids the depth of Plutarch's religiosity and the passion of his anachronistic patriotism. Embarrassed by Plutarch's spiteful treatment of Herodotus, Wardman does not even mention Plutarch's morbid interest in Ctesias' grisly descriptions of torture. Wardman's Plutarch is a tepid fellow indeed. What could Mme Roland or Jean Jaurès have seen in him?

So anxious is the author to capture the "essence" of Plutarch that he eschews the prickly problem of sources. Granted that *Quellenforschung* often resembles Alice's Wonderland, Wardman ought to have surveyed the relationship between Plutarch and his sources more thoroughly. Despite his disdain for "tragic history," Plutarch could not resist the melodramatic appeal of Duris and Phylarchus. While damning both writers, he uncritically copied from their works, and we are enriched by his inconsistency. Although the disregard for sources is a serious flaw, Wardman is helpful in pointing out the philosophic underpinnings of Plutarch: "The *Lives* are an extended meditation on Platonist themes. . . . The political life, in Plutarch's view, is a form of philosophy in action." Even Plutarch's forte, the significance of insignificant data, is based on "Plato's idea that men's amusements are a guide to their natures." In this vein, one wonders how so arid a book can be written on Plutarch's *Lives*, but the author is willing to "concede that there are as many Plutarchs as there are readers."

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G. V. SUMNER. *The Orators in Cicero's Brutus: Prosopography and Chronology*. (Phoenix: Journal of the Classical Association of Canada. Supplementary volume 11.) [Toronto:] University of Toronto Press. 1973. Pp. x, 197. \$12.50.

In the days before there were speech writers and "the media," a Roman in public life depended on his personal performance as an orator, both in political speeches and in the forensic activity that was one of the traditional ways in which a Roman with ambition for office kept himself in the public eye. Composition and delivery were discriminatively assessed by contemporaries who were pursuing the same career. Cicero's *Brutus* constitutes a unique critical history of Roman oratory from the period of the Punic Wars down to his own time, dedicated to and named for his friend Marcus Junius Brutus, himself a distinguished orator. Since Cicero describes the careers and performance of more than two hundred personages who were active in the public life of their times, the work is a precious source for modern historians. Cicero treats as a group the orators of a particular period, in the historical setting of the epoch, and, on the basis of his own judgment, evaluates the effectiveness of the rhetorical style of the period as it was employed by the various figures.

Because of loss of other evidence, the further identification of some of the orators and the establishment of the chronology of their careers have presented difficulties for historians of the epoch, crucial as it was in the history of the Roman Republic, and the scholarly literature connected with the *Brutus* has become extensive. After well-known preparatory research, Professor Sumner, of the University of Toronto, here provides a valuable synthesis of the problems and the evidence. The material is conveniently arranged in a register of orators, a prosopographical commentary on the names, a study of the chronological structure of Cicero's treatment, and an investigation of Cicero's sources for his knowledge of the orators' dates. The book is to be welcomed as an important instrument of research that must be consulted by all scholars concerned with the history of the period.

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MICHAEL GRANT. *The Army of the Caesars*. New York: Charles Scribner's Sons. 1974. Pp. xxxiv, 364. \$15.00.

This book claims to be "the first general history of the army in Roman society." That claim is staked somewhat widely. Only eighty pages of text deal with the time after A.D. 69 and only sixteen pages

of these are devoted to the last 241 years. Nor is the emphasis sociological, the main topic being the emperors' and their contenders' relation with the legions and the guard. Properly speaking, then, this is a history of the army in Roman politics during the Julio-Claudian period.

Within these limits the author's contribution is very valuable to both the general reader and the specialist, for the work is well written and the author's wide erudition in the classics and numismatics allows him to arrive at remarkable insights of a quality that often eludes the more narrowly specialized researcher. On the other hand, a work compiled chiefly from books rather than articles in scholarly journals and neglecting the fields of epigraphy and papyrology is bound to produce mistakes. Thus, military diplomas were not handed out on the Capitol but in the garrison provinces; Italians under Trajan did not constitute a mere one per cent of the legionary force, but twenty to twenty-five per cent; and *evocati* were not the veterans customarily kept with the standards for some additional years, but men singled out by the emperor to serve as officers.

The bibliography is useful but the purpose of the notes is baffling: in a book designed for the general reader one would expect them to cite modern works where the relevant documentation and discussion can be found; instead, they mostly refer to passages in the classics without any attempt to be either systematic or exhaustive—*cui bono?*

The epilogue concludes that the price for a politically active army is permanent instability and that the Roman Empire paid this price in the form of "extremely frequent and damaging upheavals." Yet, over the quarter millennium from Augustus to Caracalla there were only two armed conflagrations, damaging, but short—a record comparing favorably with almost any other type of state, ancient or modern, and disproving the suggestion that the army of the principate took politics in hand to an extent even remotely comparable to modern military regimes. Still, this is a book eminently worthwhile to read.

MICHAEL P. SPEIDEL
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GÉZA ALFÖLDY. *Noricum*. Translated by ANTHONY BIRLEY. (The Provinces of the Roman Empire.) Boston: Routledge and Kegan Paul. 1974. Pp. xxiv, 413, 1 map, 58 plates. \$39.25.

This is a handsome volume, an excellent contribution to the series. The archeological material is admirably employed, and often visually presented, in fifty-eight plates and forty-five figures. The less abundant literary evidence is used fully. The translation is smooth.

Noricum was probably the least important of the provinces along the northern frontier. Civil wars and invasions sometimes passed it by. Into some of its backwaters, influences percolated slowly. Yet Alföldy shows that Noricum was in many ways typical. Roman influence penetrated the Celtic kingdom along with traders selling pottery and other wares and buying the all-important iron, metals products, and other local goods. The kingdom became a Roman ally. Under Augustus it was taken over almost without a struggle. Claudius, especially, accelerated the process of urbanization and the development of new social classes. Noricans served in the Roman army. The citizenship was granted liberally. For more than two centuries the area was secure and generally prosperous. But then came the third century; and though there were periods of recovery, as under Diocletian and Constantine, the trend was in general downward. Or should one say upward? For just as the towns had descended from the hilltops to the plain when the Romans brought security, so when security broke down the remnants of the people fled to fortified heights to escape the Alamanni, the Huns, and the rest.

Significant political, social, economic, and cultural history is carefully extracted from the meager sources. Alföldy occasionally outstrips his evidence: a tile stamp from Juvavum hardly proves the existence of a brick import trade from Aquileia (p. 109n.), nor does the discovery of one coin hoard show "panic" (p. 153). Yet the author is usually judicious.

Some interesting facts emerge. Apparently natives sold their sons as slaves to rich Roman citizens on condition that the sons be freed at about age thirty—with Roman citizenship, of course. The practice later declined, perhaps because almost everyone after Caracalla had the citizenship.

The extensive apparatus makes the book eminently usable. There are seventeen appendixes—personal names, officials, military units, and so forth—an extensive bibliography, three indexes, and a good map.

HENRY C. BOREN
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ALBINO GARZETTI. *From Tiberius to the Antonines: A History of the Roman Empire, AD 14–192*. Translated by J. R. FOSTER. London: Methuen and Company; distrib. by Barnes and Noble, New York. 1974. Pp. x, 861. \$30.00.

The original Italian version of this book was issued in 1960 as volume 6 of the eight volumes on the political history of Rome from its origins to the fall of the western Empire, in the series *Storia di Roma* published under the auspices of the Istituto di

Studi Romani. That series contains volumes for ancient Rome on law, religion (pagan and Christian), art, literature, and topography. Garzetti therefore does not devote separate chapters to these topics, nor does he do so to social, economic, or provincial history. Naturally he introduces material from these fields in support of his treatment of the political and military history; for example, he used the columns of Trajan and Marcus as sources for their wars. The arrangement is by reigns, with a general scheme of personality, internal affairs such as the court and administration, external affairs such as wars and frontiers, and a conclusion on death and succession.

In a short review there is no space for detailed consideration of Garzetti's positions on debatable issues. He revised and added to both text and notes for this translation. In general his narrative (pp. 1-553) is clear, and his judgments on men and events are sound. He does not try to whitewash the "bad" emperors but presents from nonliterary sources, such as coins, inscriptions, and papyri, evidence for their effective contributions to government, to counterbalance the hostile senatorial tradition presented by the literary sources. The only criticisms that may in general be made about the text are that the style is heavy, the sentences are often unduly long, and no maps are provided, particularly for the wars.

Scholars will find of particular value the lengthy critical notes, both those translated from the original (pp. 557-730) and those covering 1960-69 added for this edition (pp. 731-44), as well as the full bibliography through 1969 (pp. 775-822). These notes and bibliography purport to take off from volumes 10 (1934) and 11 (1936) of the *Cambridge Ancient History*. There is, however, frequent citation of relevant material anterior to these dates. The notes present and analyze an immense range of new materials, chiefly inscriptions, and of modern literature for the years 1934/36-69. These critical notes therefore afford a rich resource of evidence, briefly but critically discussed to support the positions taken in the text.

In short no one concerned with the period from the accession of Tiberius in A.D. 14 to the death of Commodus at the end of A.D. 192 can afford to neglect this book, noteworthy for its fullness of treatment of political and military history, for its soundness of judgment, and for its wealth of evidence critically presented.

MASON HAMMOND
Harvard University

MEDIEVAL

RUSSELL FRASER. *The Dark Ages & the Age of Gold*. Princeton: Princeton University Press. 1973. Pp. xi, 425. \$16.00.

Russell Fraser, chairman of the English department at the University of Michigan, here amplifies and repeats in part a thesis that appeared earlier in his book *The War against Poetry* (1970). This new study attempts to trace the emergence of rationalism, defined as a passion for unambiguous truth, which for Fraser marks the beginning of the great divide between the past and the present. This passion he sees as contemporary with the Renaissance and the Age of Reason, labels used not to designate different chronological periods but a single age characterized by a common mode of thought. The title of his book lends itself to confusion since it is not the author's purpose to focus attention on two chronological periods of traditional nomenclature, but to examine two drastically different overlapping modes of thought discovered by his examination of selected writings throughout the course of Western history. The scope of the literary field examined is awe-inspiring, but the broad research lays a foundation upon which a narrow, far-reaching conclusion is constructed. The method and purpose of the work are declared to be historical, but with the reservation and caution that the thesis speaks "only to a corner of things" (p. x). Fraser repeatedly warns of his bias and specifically orders *Caveat lector* (p. ix).

Causal relationships are not clearly indicated in the hypothesis that offers a litany of references from Abelard to Zola, from Bach to Babbitt. Chronological and geographical references are often vague. Fraser locates the dramatic break with the medieval mode of thought in two different localities at two different periods: on the Continent during the fourteenth century and in England during the sixteenth century. He acknowledges the confusion that exists concerning the historical explanation of the emergence of the modern age but concludes that for England the source of the confusion is the presence of Shakespeare, a medieval artist—the last and greatest—in the Age of Reason. Shakespeare is characterized as atavistic and medieval because "in him is summed up wondrously the capacity to entertain concurrent truths which may be contradictory" (p. vii).

Historians, whose discipline knows no area forbidden to trespass, have a responsibility to acquire the methods and skills of the special territory that they invade. Cross-disciplinary and interdisciplinary studies are not the province of any one academic field. Certainly literature is grist for a variety of intellectual mills. Historical studies of literature such as Fraser's should be encouraged. Nevertheless every academic must employ the techniques, terminology, and methods of the discipline he chooses in his search for truth, contradictory or not. No less should be expected of a literary expert who attempts a historical synthesis designed to

interest the commuter as he journeys to work on the train.

JOHN E. WRIGLEY
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Charlotte*

VACLAV MUDROCH and G. S. COUSE, editors. *Essays on the Reconstruction of Medieval History*. Montreal: McGill-Queen's University Press. 1974. Pp. xiii, 170. \$15.00.

The eight essays in this collection, all of them except that of Robert Folz originating in public lectures delivered at Carleton University in 1965 as part of a series on the medieval world, are essays in historiography—but at more than one level. Though the rationale for doing so is not altogether clear, the editors have divided them into two groups. The first four they describe as dealing with the changing assumptions and interpretations “that have characterized the study of designated aspects of the Middle Ages.” These are essays by Norman F. Cantor on the interpretation of medieval history in general; and by Giles Constable, W. T. H. Jackson, and Armand A. Maurer on the study, respectively, of monastic history, of medieval literature, and of medieval philosophy. The four remaining they describe as presenting “in a comprehensive fashion . . . the fruits of more or less recent investigation within particular areas.” Under this heading fall essays by Folz on Charlemagne and his empire, by Sylvia Thrupp on the medieval economic achievement, by Bertie Wilkinson on the late Middle Ages in England, and by the late Vaclav Mudroch on medieval heresy.

Given the disparate subject matter of these essays and the fact that some would appear to have been left in lecture form while others show signs of having been reworked and extended, it is unusually difficult in evaluating them to advance beyond a set of highly personal reactions. If all these pieces are certainly worth reading, Wilkinson's and Mudroch's are somewhat desultory in tone, and Maurer's, however helpful, is somewhat misleading in that it leaves the reader with no sense at all of the rich outpouring of scholarly work in the last few years devoted to the philosophers and theologians who came *after* Aquinas. Of the others, I found Thrupp's unquestionably the most stimulating and Constable's, with its rich apparatus of notes, historiographically the most rewarding. Folz's essay, valuable though it is, fits ill, because of the intensity of its focus, into the general pattern of the book.

Concerning the introductory lecture, which seeks to set the tone for the series, a more extended comment is appropriate. Here as elsewhere Cantor writes with enviable firmness, detecting “schools”

of historians where one may previously have seen only individuals and apportioning praise and blame with a confidence one can admire without necessarily being tempted to emulate. Some of his claims are highly questionable. Great historians though they were, did F. W. Maitland and Marc Bloch really perceive “the interaction of all aspects of medieval life”? And would historical understanding really be enhanced if we chose to begin “the medieval era with the foundation of the Christian Church in apostolic times”? But even where he fails to convince, Cantor usually contrives to stimulate. He is a restless and highly perceptive observer not only of the medieval world but also of the foibles of modern historical scholarship. Thus, he remarks, a little wistfully it may be, that “the advance in historical knowledge depends perhaps even more upon historiographical self-consciousness than upon laborious research into new documentary sources.” Hyperbole, no doubt, but there are good grounds for believing that a better balance between the two would indeed be desirable. One would like to think that the essays printing in this volume will themselves do a modest something to help achieve that better balance.

FRANCIS OAKLEY
Williams College

JACQUES HEERS. *Le clan familial au Moyen Age: Étude sur les structures politiques et sociales des milieux urbains*. (“Collection Hier.”) Paris: Presses Universitaires de France. 1974. Pp. 272.

This is an important book that will stir up a considerable amount of controversy. The author advances two propositions: first, that family clans played an important, and often dominant, role in the political and economic life of medieval towns; and second, that because the clans were composed of or led by nobles who had extensive rural landholdings, the sharp distinction made by most historians between rural and urban societies is not meaningful. The word “clan” is defined precisely. It is not just an extended family; it is an association of rich and poor, patrons and clients, landowners and businessmen, and at times nobles and non-nobles. The members of the clan accept a common leadership, group themselves around a common territorial center, and often reinforce their secular ties through control of a parish church.

The best evidence for these theories comes from Genoa, a town where the clans were officially recognized and where their power lasted well into the early modern period. The author's unrivaled knowledge of the Genoese archives enables him to work out the most intricate and revealing details about clan organization. The evidence for the rest

of northern Italy, and especially for Tuscany, is not as full, but it still proves that the clans had considerable influence in urban government. Outside Italy the only completely convincing case is Metz. This may be, as the author says, because no one has really looked for clans in the trans-Alpine countries, although there has certainly been fairly careful investigation of the noble *lignages*. It seems more likely that the clans flourished only in the absence of a strong central government. It is hard to imagine Henry II of England or Philip Augustus of France allowing such autonomous associations to control their towns.

Two final criticisms: the author's definition of "noble" is a little imprecise (the *ministeriales* of Germany were certainly not noble when they began to play a leading role in German towns); second, in many towns, ranging from Burgos in Spain to Cologne in Germany, fraternities—to which the author gives only one page—were more influential than clans. Nevertheless, this book will make all medievalists think harder about their theories of urban development.

JOSEPH R. STRAYER
Princeton University

CHRISTOPHER BROOKE. *The Monastic World, 1000–1300*. New York: Random House. 1974. Pp. 272. \$35.00.

The Roman poet Lucretius apologized for putting his thoughts on the serious subject of Epicureanism in verse form, but he explained that if he did not sweeten what he had to say with the honey of poetry, he would have no readers. This prefatory observation is not meant to imply that Professor Brooke would have had difficulty "selling" his study of medieval monasticism had he offered it in the conventional garb of a historical monograph. What the reference to Lucretius does suggest, however, is that it is no less necessary for the historian today, than it was for the scholar back in antiquity, to present his subject in an attractive form if he is to find himself any readers.

It may have been for the purpose of reaching a wider audience that Brooke undertook this work of collaboration with the noted art historian Wim Swaan. And all of us are the richer for their efforts. By being seen in each other's company, both Brooke and Swaan have made new friends, while the reading—and seeing—public will be better able to appreciate the vocation of the monk and his place in medieval society by meeting him in his physical, and esthetic, surroundings. Brooke writes: "We have both tried to see in the visible remains of medieval monasteries the history, culture, way of life and religious sentiment which they reveal" (p. 7). As the historian carries his story of

monasticism forward from its origins through new rules and orders—Carthusians, Augustinian Canons, Cistercians, Premonstratensians, Franciscans, and others—there is Swaan at hand with his splendid photographs of monastic buildings, their interiors, sculptures, frescoes, and manuscripts, bent upon captivating the reader while at the same time illuminating the text. The striking picture of the monastery of Saint-Martin-du-Canigou in its beautiful natural setting, which graces both the dust cover and the text, is itself worth the price of the volume.

Physical remains are sparse for the early Middle Ages, but for the period 1000–1300 to which the authors give greatest attention, they abound. Here professionally and spiritually Brooke is most at home, and it is manifest from the intimate descriptions he offers of the history and building of such monasteries as Chartreuse, Maulbronn, and Mont St. Michel that he took the time to visit these and many of the other places he discusses.

To "the first and most challenging question . . . : can we fix our narrative together with the visible remains of medieval monasteries?" (p. 201), Brooke gives no convincing affirmative. But no matter; the reader will not object.

JOSEPH DAHMUS
Pennsylvania State University

R. C. SMAIL. *The Crusaders: In Syria and the Holy Land*. (Ancient Peoples and Places, volume 82.) New York: Praeger Publishers. 1974. Pp. 232. \$12.50.

Recent studies on the institutions of the crusaders' states and continuing examination of extant architectural remains and other artifacts, notably by Richard, Prawer, Riley-Smith, Benevisti, Boase, and Buchthal, have necessitated a reassessment of many formerly held views. In this book, directed primarily at the general reader, R. C. Smail, already known for his *Crusading Warfare, 1097–1193: A Contribution to Medieval Military History* (1967), presents a synthesis of such researches together with conclusions of his own, derived in part from his own visits to many of the sites mentioned. As the title suggests, this is not a history of the crusaders' states, or a study of institutions as such. Rather it seeks to portray the life of the inhabitants, conquered as well as conquerors, within the framework of their institutions and laws and against the background of their ecclesiastical, military, and civil architecture.

Smail emphasizes the role of law in crusader society, and he explains how the relations, economic and legal as well as ecclesiastical, between the crusaders and the varied elements of the native population produced a generally acceptable co-

existence. The chapters on castles, churches, and the decorative arts are well supplied with diagrams and ground plans, as well as photographs. Some of these are explained in detail. They are not, however, simply an appendix to the preceding chapters on institutions. For, whenever possible, building and art are related to contemporary political developments.

A final chapter considers whether there was a Franco-Syrian nation. Some historians have held that at least during the twelfth century a sense of crusader nationality was forming, but failed to bear fruit in the limited duration of the Latin Levant, especially after the confusion of the thirteenth century. Smail contends that even before 1187 what was developing was a Franco-Latin, not a Franco-Syrian, culture. This is evident in such essential factors as religion and language and in the continued dependence on the West, not only for trade and military support, but also for education and the development of literature. Although he has not directly considered the related question whether the crusaders' settlements were colonies, he has presented much of the material on a subject that has recently been re-examined by Joshua Prawer.

In a book that has consistently used an interdisciplinary approach, it might perhaps have been well to explain more fully the provenance of the thirteenth-century law books. The same could be said of the continuations of William of Tyre's history. Nevertheless, within the limits assigned, Smail has chosen his illustrations well and achieved his purpose of producing a work that will interest not only the general reader, but the perceptive advanced student as well.

The book includes seventy photographs, thirty-three line drawings, three maps, two genealogical tables, a selected bibliography, and an index. A recent work of a somewhat similar nature should be included in the bibliography: T. S. R. Boase, *Kingdoms and Strongholds of the Crusaders* (1971).

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W. J. FRANK DAVIES. *Teaching Reading in Early England*. New York: Barnes and Noble. 1974. Pp. xii, 211. \$14.00.

Davies interprets his subject so broadly that much of his material bears only tangentially on the teaching of reading—for example, the history of the alphabet or the discussion of pirated editions of Elizabethan texts—but his interest in all aspects of education should help to make the book interesting for a beginner in the subject. The book opens with Roman Britain and ends with 1612, but the treatment of the first millennium is necessarily

sketchy, for, except by inference, we know next to nothing of how Anglo-Saxons were taught to read and not much more about teaching under the Normans. There is a good deal more evidence for the later centuries, particularly the sixteenth, from which we have primers and the writings of Roger Ascham, Richard Mulcaster, and others.

As Davies points out, the problems (for example, the age for beginning reading) and methods (alphabetic, phonic, and so forth) that are still debated today were debated in the Renaissance. Educational theory appears to go round in circles. Fortunately the young mind is usually flexible enough to learn to read, no matter what educational hobbyhorse the teacher happens to be riding.

There is no bibliography, but there are fairly copious notes. The references seem a little old-fashioned. For example, there is no reference to M. L. W. Laistner's *Thought and Letters in Western Europe, A. D. 500-900* (1931; rev. ed., 1957).

Davies shows a commendable interest in what there was for the pupil to read after he had learned how. One reason for the low rate of literacy in the age of manuscripts must have been the paucity of reading material. The book may be useful for one interested in general history of English education to 1612 or in the teaching of reading in the Renaissance. The expert in the Anglo-Saxon period will find nothing new. I cannot speak for the specialist in Renaissance education.

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Boulder

MICHAEL R. POWICKE. *The Community of the Realm*. (The Borzoi History of England. Volume 2: 1154-1485.) New York: Alfred A. Knopf. 1973. Pp. xii, 194. \$6.95.

In his foreword to this second volume of a five-volume Borzoi History of England, Arthur J. Slavin, the editor, tells us that no one methodology or school of ideas has set the mold of this collaborative effort, that each author has written his volume according to "the drum each heard best" (p. viii). The beat of Professor Powicke's drum has him looking at English history between 1154 and 1485 "as the history of communities. Beneath the over-arching community of king and court—including the higher aristocracy—was a diverse constellation of communities that embodied the lives and aspirations of the people" and that "nurtured a culture, a form of conscious expression of attitudes and ideas" (pp. 10-11). As those devotees of English medieval history know, the drumbeat heard by Powicke is the sonorous and majestic beat of William Stubbs à la Bertie Wilkinson.

That this book has, therefore, a point of view, an ingredient lacking in most eclectic surveys and texts, is all to the good because the reader is presented with a succession of ideas and interpretations with which he can agree or disagree.

Following a useful introductory chapter on historiography from the Tudors to the present is a long, and incidentally the best, chapter in the book, "The Monarchy and the Community of the Realm." Only slightly concerned with political history from Henry II to Bosworth Field, it concentrates upon kingship, war, foreign policy, finance, royal government, justice, council, and Parliament. An expert on medieval war and institutions, Powicke presents a clear account of the functions, objectives, and growth of these parts of royal and central government. He does this under the ideological umbrella that "there developed a body of men at the center of national affairs who felt themselves specifically the trustees of national interests: they were 'the community of the realm'" (p. 52). Throughout this period when the common law and Parliament were taking their shape "cooperation was stronger than conflict, especially under a king who was militarily successful and in harmony with the spirit of his greater subjects" (p. 52). This optimistic view of English medieval political life occasionally leads Powicke to use words and to make statements inexact or awkward to defend. He uses "nation," "national," "national forum," "people's preference," and "popular leaders" frequently and loosely for such a period. He asserts that Richard II appeals to moderns because he made peace abroad and cherished the arts, that the average Englishman never had it so good as during the first half of the fifteenth century, that Henry III was a calculating administrator and something of an executive genius, that in the late fourteenth century Gaunt and Gloucester were military giants, and that those men who came to constitute Parliament were "proud and self-confident men" (p. 71). For such statements Powicke has insufficient or no evidence.

The chapter on local communities describes in a workmanlike fashion the household, manor, vill, and town, but most scholars have long abandoned the idea that the "continental medieval town was a 'collective fief'" (p. 95). Some seventy-five years ago Henri Pirenne effectively destroyed this theory of Arthur Giry and Achille Luchaire. Only because James Tait found it an attractive idea do some English medievalists still cling to it.

In the chapter on church structures Powicke remains Stubbsian in his view of "the English church" and categorically asserts that "the existence of an *Ecclesia Anglicana* with its own identity and practices was generally accepted" (p. 105). This statement would surely annoy F. W. Mait-

land, considering all that he did to demonstrate the contrary.

The final chapter on intellectual and cultural history reveals the affinity of Powicke for things and accomplishments English. Imagine the reaction of French, German, and Italian medievalists to the following: "England was in the mainstream of the development of Western culture. . . . England's scholars and writers contributed to European thought on a scale out of proportion to its population and wealth" (p. 135).

Useful, with an appendix on primary sources, a discriminating bibliography, and a glossary of technical terms, this book presents views often unconvincing but ripe for controversy. To see how students react to it will be interesting.

BRYCE LYON
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ARTHUR STEPHEN MCGRADIE. *The Political Thought of William of Ockham: Personal and Institutional Principles*. (Cambridge Studies in Medieval Life and Thought, Third Series, volume 7.) New York: Cambridge University Press. 1974. Pp. xiii, 269. \$18.50.

This is an original and authoritative exposition of the theories of the fourteenth-century Franciscan, William of Ockham, on the basis and function of secular authority and on ecclesiastical authority, power, and government. McGrade has given us not only a thorough and judicious discussion of the present—and confused—state of Ockham scholarship, but with unusual skill he has delineated the relationship between Ockham's speculative and political thought, always concentrating on the significance of those political ideas in their proper historical context. Ockham emerges from this study not only as the intellectual destroyer of the medieval Augustinian world order, which has long been argued, but as a political thinker much in advance of modern political and social practice as well.

McGrade traces Ockham's involvement in politics to his staunch conviction of the heresy of Pope John XII, discusses Ockham's support for his erstwhile protector Ludwig of Bavaria and for Edward III of England, and emphasizes Ockham's *Dialogus III* as the most systematic political treatise he composed. A fundamental characteristic of his English mind, one common throughout all of his writings, was the use of carefully constructed legal arguments to defend opposition to established legal authority.

Just as E. F. Jacob once observed that, "there seems to be no way round Ockham" and that "sooner or later he confronts every worker in late

medieval history," so, many historians and political scientists will discover the necessity of consulting this book. It is soundly researched, coherently organized, and written with a lucidity rare for so technical a subject.

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ALBERT BOURGEOIS. *Lépreux et maladreries du Pas-de-Calais (X^e-XVIII^e siècles): Psychologie collective et institutions charitables*. Preface by MARCEL BAUDOT and MARCEL CANDILLE. (Mémoires de la Commission Départementale des Monuments Historiques du Pas-de-Calais, volume 14, part 2.) Arras: the Commission. 1972. Pp. 358, 12 plates. 60 fr.

The scope of its title notwithstanding, this work is essentially a closely documented survey of leprosaria in the Pas-de-Calais. It reaches back to the tenth century for the foundation of the earliest of these institutions and forward to the eighteenth century for evidence of their eventual dissolution, but the emphasis of the presentation is on the twelfth through fourteenth centuries, the period during which leprosy was most widespread in Europe. The incidence of the disease declined beginning in the fourteenth century and experienced a brief recrudescence in the early sixteenth century, but by the end of the seventeenth century most leprosaria were vacant of lepers and many were in ruins. Those that remained were made into hospitals or annexed to the new *hôpitaux généraux* created by Louis XIV.

In the history of charity and public assistance, the leper forms an important chapter. Although in medieval times he was the "malade par excellence" of Europe and gave his name in France to the institution created for him, the *maladrerie*, he was nevertheless distinguished in several important respects from other kinds of sick persons. The leper represented a threat to public order and public health before he concerned authorities as an object of charity. It is not surprising to learn therefore that most leprosaria in the Pas-de-Calais were of seigneurial and communal foundation and administration, rather than of ecclesiastical origin and control, as were most other charitable institutions in this period.

Furthermore, the leper was alone among objects of charity in that his condition was not invariably accompanied by poverty. Leprosy struck all classes of society. Not a few of the seigneurial foundations were due to personal or family experience. Leprosaria in the towns were often restricted to members of the native bourgeoisie, and some of these institutions were remarkably well endowed with buildings, lands, and goods. Mr.

Bourgeois is careful to observe that we know more about these bigger, often urban, leprosaria than we do about the smaller countryside shelters, or about the lepers who belonged to the lowest classes of society and were often mendicant, for neither of these latter has left sufficient records.

The Pas-de-Calais seems to have been distinguished both by the number of leprosaria it possessed (the list includes over 170, although most were small country refuges) and by the richness of many of those institutions whose records have survived. Bourgeois estimates that about thirty establishments possessed chapels and some produced enough income to enable their administrators to lend out money.

The introductory remarks that preface the survey refrain from drawing more from the documents than they will bear, and the survey itself, with its extensive notes, is a most valuable source for the early character of public assistance in the specialized form it took toward the leper.

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WALTER L. WAKEFIELD. *Heresy, Crusade and Inquisition in Southern France, 1100-1250*. Berkeley and Los Angeles: University of California Press. 1974. Pp. 288. \$14.00.

In *The Medieval Manichee* (1947), Sir Steven Runciman described "the Christian Dualist Heresy." To compare Runciman's work to Professor Wakefield's would be a little like comparing an Italian "primitive" painting, with no sense of perspective, to a painting of the later Renaissance. The parallel, if unfair, may indicate the progress achieved by recent research. Wakefield's description of "heretical" views and practices, notably Waldensian and Cathari, can draw on the work of Herbert Grundmann, Christine Thouzellier, and many others. Whereas Runciman saw "one great confederate Dualist Church stretching from the Black Sea to Biscay" (p. 171), Wakefield sees the rise of Western heresy as inspired by a general revival of religion; the "apostolic ideal" of holy poverty as behind both dissident and orthodox reform. The "Gnostic tradition" and Paulician missionaries from Constantinople are not needed to explain native criticism of ecclesiastical worldliness. Contacts established about 1150 between the Bogomils of the Balkans and the West do not seem as crucial as was previously thought. Even after many Cathari had adopted strongly dualist beliefs, their main appeal continued to be to the New Testament and to the contrast between the lives of the Cathari perfect and the contemporary Catholic clergy.

Wakefield's discussion of the southern French

social and ecclesiastical background avoids simplifications found in other authors, such as the identification of heresy with one social class or the idea that heretics ever formed the majority of the population. Here Wakefield is much indebted to the works of John H. Mundy on Toulouse. There follow unusually balanced accounts of the Albigensian Crusade, with its political sequel, the gradual incorporation of Languedoc under the direct authority of the French Crown, and of the beginnings of the Inquisition. The appendixes contain translations of contemporary sources, the most important being the vivid narrative of the early Inquisition by William Pelhissou. These translations supplement *Heresies of the High Middle Ages* (1969), edited by the late Austin P. Evans and Wakefield. The very full bibliography completes an extremely useful volume, which fills a long-felt gap in scholarship in English.

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HELMUT LIPPERT. *Thietmar von Merseburg: Reichsbischof und Chronist*. (Mitteldeutsche Forschungen, number 72.) Cologne: Böhlau Verlag. 1973. Pp. vi, 245. DM 36.

The purpose of this extended dissertation is a revision of the older "static" analyses of Thietmar's attitudes and motives for writing his chronicle. The author acknowledges the merits of his predecessors in the subject, like those of Robert Holtzmann, Karl Erdmann, and Anton Schneider, but proposes that the apparently heterogeneous elements in the world view of the eleventh-century author can be easily integrated. It has been said that Thietmar was at the same time a prince of the Church and a devoted holy man, a territorial lord and a good shepherd of his flock, a nobleman loyal to his king as well as to the Church, but also bound by family ties and clan solidarity. These terms neatly outline the "type" of the high medieval imperial bishop, however incompatible some of them may appear to us. The author of this book did not only wish to render obsolete such propositions as that of "two spirits fighting in Thietmar's soul" but also to reply to the question regarding the origin and background of all these elements in the chronist's work.

The book opens with a chapter on the three historical factors that defined Thietmar's world: the aristocratic domination of the early medieval Church, the office of the medieval bishop, and the specific combination of both in the "Ottonian system." Of course, on barely forty pages he can only offer generalities. The bulk of the text (pp. 46-137) is devoted to the correlations between Thietmar's biography and horizon of experience—and his

chronicle. Beginning with the aristocratic background through education and spiritual preparation to the official career of the bishop of Merseburg (1009-18), the author demonstrates the roots of Thietmar's information, biases, and judgments. He certainly knows his text well and also the commenting literature, but sometimes one would wish a somewhat less literal procedure from the "human conditions" to the written text. While it is, for example, certainly appropriate to confront documentary evidence on the quarrels between episcopal sees with Thietmar's text, such mental characteristics as "noble loyalty" and "clerical solidarity" are not so readily recognizable in selected passages. The third part of the book analyzes Thietmar's assessment of the five kings he is dealing with, from Henry I to Henry II (pp. 138-92), and compares his chronicle with writings originating in the circles of Liudolfing "family tradition." While the imperial bishop displays a more dogmatic view of kings and kingship than the "court authors" of the Ottonian dynasty, he, too, is deeply indebted and partial to his imperial patrons. The epilogue points to an important aspect of the chronicle that was less emphasized hitherto: its "memorial" character. The oft-quoted self-recriminating sections in Thietmar offer the best hints at this tendency: the writer is troubled by demons and ghosts but finds peace in securing the individual and collective memory of departed *confratres* and benefactors. His writing of history is to a great extent motivated by this endeavor. Two excursions are added: one on the worship of the Baptist in the Ottonian dynasty and another on the legendary afterlife of Henry the Fowler. The transformation of the epitheton from an originally obscene reference to the "lustful king"—which found its reflection, though displaced, in Thietmar—into the harmless, though socially uncomplimentary *auceps* is nicely elaborated, together with other blendings of history and folklore. Maybe a wider discussion of some lesser-known topics—as these last ones—would have deserved more detailed treatment, even at the expense of the sometimes repetitive reflections on the "influences" that formed Thietmar's mind and writing.

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DAGMAR UNVERHAU. *Approbatio—Reprobatio: Studien zum päpstlichen Mitspracherecht bei Kaiserkrönung und Königswahl vom Investiturstreit bis zum ersten Prozess Johanns XXII. gegen Ludwig IV.* (Historische Studien, number 424.) Lübeck: Matthiesen Verlag. 1973. Pp. 418. DM 76.

Dr. Unverhau has produced a monograph—an expanded version of her doctoral dissertation written

under Professor Otto Brunner—that will have only a limited appeal to the general student of the Middle Ages. The subtitle of the work (in translation), “A Study of the Papal Right of Equal Participation in the Imperial Coronation and Royal Election from the Investiture Struggle to the First Charge of John XXII against Ludwig IV,” sums up the focus of the work. As far as I can determine, the term “Mitspracherecht” may well be a juridical term that has taken root in German thought only in the last generation since, in the sense of “equal participation,” it is absent from standard translating dictionaries.

After a brief glance at the beginnings of papal-Frankish relations, the main thrust of the study begins with the position taken by Gregory VII in 1076 and moves systematically through the first phase of the interminable contest between John XXII at Avignon and Ludwig the Bavarian. Thus the study covers the period of the Gelasian two-sword doctrine through the development of the theory of papal monarchy in the curia while in the Reich the imperial ideas move through the rediscovered Roman law to natural law to the concept of the secular imperium.

For the student interested in procedure, in precise terminology, in the shifting of the grounds of argument in the century and a half after 1076, the present work covers at length the arguments of the legists and the canonists as well as provides a running commentary and analysis of the positions taken by the modern interpreters of this process. While Unverhau's study ends before the Declaration of Rense in 1338, she has traced the logic that would end in the new posture of state and Church, rather than the medieval one of Church and state.

SCHAFER WILLIAMS
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WALTER HÖFLECHNER. *Die Gesandten der europäischen Mächte, vornehmlich des Kaisers und des Reiches, 1490–1500.* (Österreichische Akademie der Wissenschaften Philosophisch-historische Klasse, Historische Kommission. Archiv für österreichische Geschichte, number 129.) Vienna: Hermann Böhlhaus Nachf. 1972. Pp. 490. DM 110.

This compendium, the author frankly states, originated as a research tool for a major study of diplomatic developments concerning Emperor Maximilian I and the Holy Roman Empire during the last decade of the fifteenth century. And it is indeed a most useful research tool. The author provides us with a listing, ordered by states, of all ambassadors and embassies, negotiators, heralds, and others who appear in the historical literature and in the available documents during this period.

In addition, the author gives, whenever possible, some biographical details regarding the major figure in each diplomatic mission, together with a brief résumé of the mission itself, and finally he supplies a brief annotation where the diplomat or the mission is mentioned in the historical literature or in the documents.

The basic sources for this compendium and repertory of a decade of European diplomacy are the as yet unpublished *Regesta imperii XIV—Maximilian I*, assembled by the historical institute of the University of Graz under the direction of Professor Hermann Wiesflecker. For his published sources the author relies on a vast array of works, primarily the *Relazioni* edited by Eugenio de Alberi, the Habsburg documents edited by Joseph Chmel, the diaries of Marino Sanuto, and the *Calendar of State Papers*. When primary sources, either manuscript or printed, were unavailable, Dr. Höflechner utilized standard diplomatic and other accounts such as Eduard Fueter, Karl Fischer, Johann W. Zinkeisen, and others.

This is a difficult compendium to use, but it contains a great amount of painstaking and accurate work. It is not an introduction to the diplomatic history of the period, but rather a reference work and a research tool. Given the depressed state of scholarly publication these days, the Austrian Academy of Sciences Historical commission, which provided the necessary financial backing for this volume, is to be most highly commended.

GUNTHER E. ROTHENBERG
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PIERRE TOUBERT. *Les structures du Latium médiéval: Le Latium méridional et la Sabine du IX^e siècle à la fin du XII^e siècle.* In two volumes. (Bibliothèque des Écoles Françaises d'Athènes et de Rome, fascicule 221.) Preface by OTTORINO BERTOLINI. Rome: École Française de Rome. 1973. Pp. xxvii, 787; 790–1500, 7 maps.

In this lengthy thesis Professor Toubert examines the changing social and governmental structures of southern Latium and the Sabine region of central Italy, to the south and east of Rome, across the central Middle Ages. He maintains that “the great fact” in the history of Latium from approximately 800 to 1200 was *incastellamento*, the proliferation of castles from the early tenth century. This “castle revolution,” as Toubert calls it, involved far more than the fortification of old or new centers of settlement. All other “structures” of medieval Latium were affected by it: the nature of settlement; the character of the rural economy; roads, markets, and exchange; the family; the organization of the Church; and the administration of justice. In regard to the rural economy, for example, Toubert

concludes that in the ninth century agrarian settlement was dispersed and open, and the peasant was free to expand his holdings at will into the wasteland. After *incastellamento*, settlement became closed and concentrated, and the peasant was burdened with heavy seigneurial exactions. Toubert suggests that the ninth century, in spite of occasional Saracen incursions, was a "golden age" for the Latian peasant, who enjoyed psychological comfort and security under the efficient but loose Carolingian regime. He affirms that "in the eighth and ninth century, the State did exist." The revolution of the tenth century thus marked not a renaissance, as Robert Lopez would maintain, but a "qualitative break" imposed upon an older growth. "The *castrum* marked the end of pioneer life," and it also allegedly undermined the preceding "rural happiness."

The novelty of these views will at once impress medievalists. Where others have seen impoverished serfs settled on overcrowded Carolingian manors, Toubert discerns free and open settlement and much *bonheur rural*. Where others speak of a tenth-century renaissance and the formation of active frontiers, he locates in the same period "the end of pioneer life." In support of his often audacious positions, Toubert maintains a nearly continuous polemic against those scholars who may have once expressed differing views. Given the character of his thesis, the scholars he chooses to attack are many, and the sharp terms applied to their work—*illusoire*, *bien des contresens*, *non-sens*, and the like—become a little disconcerting.

Toubert's great energy, competence, and high critical acumen lend lasting importance to his work. But the book also manifests several serious flaws. It is much too long. Perhaps there is something in the logic of the French *thèse* that requires the author to strive for comprehensiveness and hence to propose and discuss topics about which little can be known. Typical of much of the book is a section beginning on page 1330 where we are told that the "pénurie documentaire très marquée" makes necessary "beaucoup . . . de brièveté dans l'exposé." The section nonetheless runs on for nine pages and is chiefly composed of strings of unanswered questions. Other sections, also dealing with poorly illuminated topics, end in trivial or banal conclusions (see what is said about the "castle revolution" and the family, pp. 714 ff.).

On a more substantive level, reservations are also permissible concerning the central thesis of the book, the "castle revolution" and the sweeping changes attributed to it. We are told almost nothing about who built the castles and why. The author, over the many years spent in composing the book, seems to have altered his own inter-

pretations as his work progressed. At all events, the reader must puzzle how *incastellamento* can be called "une croyence" (p. 305) and yet be said to mark the end of the "vie pionnière" (p. 1357). At times the "castle revolution" is depicted as slow, gradual, incomplete, and quickly surpassed by other changes (p. 493). We learn that "many castles . . . quickly degenerated into isolated large farms," or were entirely abandoned. On page 733 the author speaks of the "fragile stability of the castle structure." Is *incastellamento* really the powerful motor of revolutionary change, transforming all that it touches, or only a short-lived phase in a larger process—the construction of the Papal State?

The relationships alleged by Toubert between *incastellamento* and settlement are especially perplexing. It must first of all be recognized that the documents—the richest of them are associated with the monastery of Farfa—are often vague or ambiguous, and it is nearly impossible to track individual farms or holdings over time. "Open and dispersed," "concentrated and closed"—the choice of such terms rests in large part on Toubert's own impressions. Other interpretations are possible. In the supposedly happy ninth century, impoverished peasants appear in the records of Farfa accepting land under onerous terms from the monastery, including heavy obligations in labor. Is this an example of free and open settlement? To peruse the documents of the same monastery in the twelfth century is still to encounter many references to hamlets (*casali*), which appear more often than explicit references to castles. The most frequent place reference in the documents remains, however, the vague term *vocabulum*. The castles are surely there, but their impact upon settlement is far from clear. Essentially, Toubert proposes a problem involving quantities and proportions: what proportions of the community came to be settled within or close to castles, as opposed to other centers—*casali*, villages, towns, and the like? An impressionistic reading of the sources can give a reliable answer to that question; some sort of quantitative index must be attempted. I found other conclusions advanced in this book questionable, but there is no space in a short review to discuss them further. Toubert has, in sum, produced a rich, serviceable, and stimulating, but also in parts a controversial study.

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JOHN HOLLAND SMITH. *Francis of Assisi*. New York: Charles Scribner's Sons. 1972. Pp. 210. Cloth \$8.95, paper \$2.95.

Smith's book makes no attempt to advance research on Francis's life and ambience or, indeed, to sum up its recent findings—A. Fortini, for example, is cited once and K. Esser not at all. Smith offers instead a readable biographical essay, mercifully neither edifying nor sentimental, on the saint and his image. Smith bases his account mainly on Thomas of Celano but sometimes cites Bonaventure, the *Legend of the Three Companions*, the *Fioretti*, and, more rarely, *The Mirror of Perfection*. (Apparently R. Brooke's edition of the *Scripta Leonis* was published too recently for him to use.) Of modern writers the chief influence on his interpretation is C. C. Jung. Smith connects Jung's concept of Francis with Celano's: "It was very important that Francis saw himself as the *Imitator Christi* in his generation; it was equally important that Thomas of Celano also saw him in that light, and presented him to later generations as what Jung called 'the Cosmic Man, the Anthropos.'" His life had a mythological aura, as when he withdrew to a cave in preparation for his conversion. Here, Smith says, "the story reaches back beyond traditional Christianity to the mysteries of paganism, and especially those at [sic] Pythagoras and Orpheus, and Isis/Osiris/Serapis, in which the novice 'hearer' became an adept by spending a period alone in a dark place, returning to the womb, . . . and discovering there the great treasure of enlightenment and rebirth to eternal life." Even the description of Francis's marriage to the Lady Poverty in the *Sacrum Commercium* suggests to Smith "many of the mysteries of classical and post-classical times." For him the essence of Francis is his universality, which he sees as somehow stamped by the reception of the stigmata. At that point "Francis had become the embodiment of the anthropos archetype."

Smith seems more interested in the "archetype" than in the individual. His characterization of Francis is summary. He represents the saint as first a dropout and rebel who behaved abominably to his father because he was still an adolescent at twenty-five. He and his disciples were "hysteries and fanatics by nature." As for Clare, because she had been unable to marry Francis, "she had married Christ and had become the embodiment of the Lady Poverty for whom he fought all his battles." No serious effort is made to substantiate these insights by factual evidence and psychological analysis. Moreover their connection with the portrait of Francis as anthropos is unclear.

Unclear, too, is Smith's assessment of Francis the religious leader. On the one hand he appears to follow P. Sabatier in feeling indignant about the alleged falsification of Francis's intentions by prelates outside the order and "intellectuals and budding capitalists" within it. On the other hand he

suggests that realistic leadership could never come from Francis's literal-minded fanaticism. "The later history of the Franciscan Order shows how impossible Francis's ideal was as a practical way of life for a large number of men." Even with Francis, can one always have everything both ways? Smith makes assumptions that not only are doubtful in themselves but also appear to contradict each other. At various points in his book Francis is portrayed as the universal anthropos and as a narrow fanatic, as a courageous rebel against contemporary society and as a submissive victim to ecclesiastical slyness and authority. Smith does not try to explain these paradoxes. One wonders whether he is aware of their existence.

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EUGENE L. COX. *The Eagles of Savoy: The House of Savoy in Thirteenth-Century Europe*. Princeton: Princeton University Press. 1974. Pp. xiii, 492. \$22.50.

With this volume the author of *The Green Count of Savoy* (1967) pushes his study of Savoy into the thirteenth century, with the story of the seven sons and two daughters of Count Thomas I of Savoy (d. 1233), the last of whom died in 1285. And what medieval historian has not heard of Beatrice of Savoy, mother of four queens? Cox states his aims clearly: "The creation of the medieval Savoyard state has almost always been treated as the work of its successive counts, not as a *family* enterprise, yet as I hope to show, that is the most striking feature of its history once the multitudinous activities of the family have been brought to light. Nor has the role of the House of Savoy in the broader context of European medieval history been the subject of very much study." In a work strongly supported by archival research, Cox achieves his aims admirably within the confines that he sets for himself—essentially those of "international war and diplomacy." In one sense this book is a traditional diplomatic and political history and demonstrates both the strengths and weaknesses of the genre.

While one is indeed besieged by a welter of names, places, treaties, marriage alliances, and so forth, the diligent reader will be pleasantly rewarded, for the book contains numerous and at times surprising nuggets that are well worth uncovering. Cox adds to our knowledge of important issues and historical episodes in which the almost ubiquitous Savoyards were involved. Chapter 6, revealing the nature of Savoyard involvement in French and English diplomacy in the mid-thirteenth century, is particularly valuable for its description of the Savoyard role in Henry III's ill-

fated Sicilian venture. Chapter 7 analyzes the Savoyard participation in English politics and diplomacy during the period of baronial reform. The story of the struggle between Emperor Frederick II and Pope Innocent IV is enriched by Cox's analysis of Savoyard skill in playing both sides of the street. The efforts of two episcopal Savoyard brothers loyal to the pope and of two lay brothers loyal to the emperor resulted in the protection of the totality of interests of the family of Savoy, which escaped harm from either of the major participants while reaping rewards from both. Cox offers the first detailed study of Savoyard attempts to control and exploit the western Alpine passes into Italy. His exposition of Alpine institutional reforms that laid the groundwork for the fourteenth-century Savoyard Alpine state reveals an adaptation of the office of the *bailli* created by Philip Augustus of France that provides an interesting example of the spread of institutional innovation. Several pages recounting the activities of the countess of Flanders and her Savoyard consort allow intriguing glimpses into the roles that a woman could play in government and diplomacy (pp. 100–02).

Not all will wish to read this work in its entirety, but those concerned with almost any of the major Western European political and diplomatic issues of the period with which Cox deals can only overlook this rich study at their own peril.

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WALTER BRANDMÜLLER. *Das Konzil von Pavia-Siena, 1423–1424*. Volume 1, *Darstellung*; volume 2, *Quellen*. (Vorreformationsgeschichtliche Forschungen, 16.) Münster: Verlag Aschendorff. 1968; 1974. Pp. 289; xiv, 477. DM 48; DM 98.

The first volume of Brandmüller's work on the Council of Siena was reviewed in *AHR*, 75 (1969): 109–10. The author promised a supplementary volume of selected source materials, which he has now provided in the present work. Brandmüller presents first the legislation of the council, a thin harvest of only six decrees, and then prints forty-eight papal letters, some of them previously unpublished. In the next section of the book he prints four hitherto unpublished sermons delivered at the council, three by John of Ragusa, the other by Jerome of Florence. John of Ragusa emerged as a major conciliar thinker at the Council of Basle, and it is useful to have this evidence of his attitudes to pope, council, and Church reform a few years earlier. Jerome's sermon provides an interesting example of humanistic papalism. In the final section of his book, more than half the total volume,

Brandmüller provides the first printed text of the "Protocol" of Guillermo Agramunt. Agramunt was a Spanish notary who served at the council as secretary to the embassy of King Alfonso V of Aragon. His "Protocol" is an account of the conciliar proceedings, richly documented with copies of letters and speeches that often survive only in this work. It is an invaluable source for the history of the council.

Brandmüller's first volume was generally welcomed as the first adequate account of the Council of Siena. It was especially strong in the area of diplomatic history. The author also put forward a controversial argument maintaining that the failure of the council was not due to the intransigence of Pope Martin V—as has usually been supposed—but to the maneuverings of King Alfonso V. The evidence offered in the present volume does not settle this question, but it provides valuable new source material for scholars who are interested in it and, indeed, for all students of fifteenth-century diplomacy and church history.

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M. N. TIKHOMIROV. *Rossiiskoe gosudarstvo XV–XVII vekov* [The Russian State during the 15th–17th Centuries]. (Akademiia Nauk SSSR, Otdelenie Istorii, Arkheograficheskaiia Komissia Arkhiv SSSR.) Moscow: Izdatel'stvo "Nauka." 1973. Pp. 420.

N. N. POKROVSKII. *Aktovye istochniki po istorii chernosozhnogo zemlevladieniia v Rossii XIV–nachala XVI v.* [Documentary Sources on the History of Taxable State Ownership of Land in Russia from the 14th to the Beginning of the 16th Century]. Edited by M. N. TIKHOMIROV. (Akademiia Nauk SSSR, Sibirskoe Otdelenie, Institut Istorii, Filologii i Filosofii.) Novosibirsk: Izdatel'stvo "Nauka." 1973. Pp. 229.

Both works are dated. This posthumous volume by Academician Michael Tikhomirov, who died in 1965, contains twenty-three items, twenty of which were previously published. The Pokrovskii study was a dissertation submitted to Tikhomirov in 1956 and published only now without revision. The editors of the Tikhomiroviana chose illustrative items from the entire gamut of this scholar's studies, ranging from terminological and literary discussions to historical geography. Some of the earlier pieces, dating from the 1920s and 1930s and published in Russian provincial journals, are particularly welcome. Among these are three works on Dmitrov, including a history of the town, the settlements in the surrounding district in the fifteenth and sixteenth centuries, and a study of the population changes in the subsequent two hundred years. Tikhomirov later incorporated por-

tions of these works in his historical-geographical studies of Muscovy. Three provocative items form an appendix to the volume. A short unfinished piece, on the Italians who came to medieval Muscovy, contains no new information but is distinguished by Tikhomirov's desire to restore perspective to foreign influence at the court of Ivan III. "The Feudal Order in Russia," which appeared in 1930, is virtually unknown in the West. Impressed by Pavlov-Silvanskii and others, who posited a form of feudalism in Russia akin to that invented by modern scholars for the medieval West, Tikhomirov developed a perceptive analysis of the Russian analogue. While it confuses feudalism with manorialism, the essay is sufficiently stimulating to raise questions concerning the institutional similarities and differences between medieval Europe's east and west. As such, it is worthy of translation. The last offering contains five interesting lectures given at Moscow University to students who were preparing for archival work. Tikhomirov analyzed the classes of documents kept by government bureaus in the seventeenth century, the pitfalls to be avoided, and the uses to which the evidence can be put. I know of no other guide to the study of the seventeenth-century *prikazy* papers as interesting and as sensible as this.

Pokrovskii seeks to chart the fate of the lands occupied by the Muscovite peasantry, but owned by the princes, from the fourteenth to the early sixteenth century. He discusses the encroachments by aristocratic landlords and clerical institutions upon peasant-occupied lands, the unification of the northern principalities by Muscovy, and its impact upon military servitors and agriculturalists. The last of three chapters contrasts the freedom of the peasants living in the forested frontier of the north with the fate of those in the central provinces. The work was based on laborious archival study, but since its completion most of the documents have been printed. Pokrovskii made some attempt to add the published references to his footnotes, but these are incomplete. The study would have been much improved had Pokrovskii revised his dissertation. I regret that he disregarded many of the findings of Stepan Veselovskii in his magisterial study on landholding (1947), and a considerable body of literature that appeared after 1956, notably the works of Wilhelm Schulz and Sergei Kashtanov on immunities (1965, 1967) and by Carsten Goehrke on abandoned lands (1968).

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JAROSLAV PELIKAN. *The Christian Tradition: A History of the Development of Doctrine*. Volume 2. *The Spirit of*

Eastern Christendom (600-1700). Chicago: University of Chicago Press. 1974. Pp. xxv, 329. \$16.50.

It is always difficult to give a fair assessment of a monumental work before the monument has been completed. This book is the second volume of a projected five-volume series, one of which, *The Emergence of the Catholic Tradition (100-600)*, has already been published, and three others, *The Growth of Medieval Theology (600-1300)*, *Reformation of Church and Dogma (1300-1700)*, and *Christian Doctrine and Modern Culture (since 1700)*, are forthcoming. It is the author's expectation that each volume will be able to be read independently, as well as in series. Volume 2 continues the narrative begun in the first volume through the Byzantine, Syriac, and early Russian development to the end of the seventeenth century. Highlighting of continuity and change marks the approach, though the style does not make for particularly easy reading. Each page is closely printed and the left-hand margins indicate, in abbreviated form, the sources from which the information has been drawn. This particular volume will be important for specialists in Christian history and theology who persist in neglecting Eastern Christendom but are willing to learn and for those others who need an overview of Eastern Christianity in order to understand Byzantine, Syriac, and Slavic civilizations.

The organization of the material is topical rather than strictly chronological, and the approach is theological rather than strictly historical, but the author is definitely committed to understanding and expounding the Eastern point of view from its own sources and its own spokesmen and in its relation to others. The six basic chapters survey, first, the authority of the Fathers in the areas of the changeless truth of salvation, the norms of traditional doctrine, the councils and their achievements, and knowing the unknowable; second, the union and division in Christ as related to the duality of hypostases, the one incarnate nature of God the Logos, actions and wills in unison, and Christ as the universal man; third, images of the Invisible in terms of images graven and ungraven, images as idols and as icons, and the melody of theology; fourth, the challenge of the Latin Church in expounding the orthodoxy of old Rome, the foundation of Apostolic polity, the theological origins of the Schism, and the filioque; fifth, the vindication of Trinitarian monotheism, including a discussion of the Trinity and Shema, evil and the God of Love, the One God—and His Prophet, and the God of the philosophers; and sixth, the last flowering of Byzantine Orthodoxy, embracing the mystic as New Theologian, the final break with Western doctrine, the definition of Eastern particularity, and Slavic Orthodoxy.

The Spirit of Eastern Christendom—so titled as to include non-Chalcedonian Orthodox and other Eastern Christians as well as the Orthodox Christian majority—clearly shows how Eastern Christendom was constantly being challenged by all kinds of religious, political, military, and ideological factors to interpret and refine Christian doctrine. Professor Pelikan, who is actively engaged in theological and ecumenical discussions, is acutely aware of the necessity of the Eastern orientation for a proper understanding of the whole of Christianity. The significant differences in cultural orientation between East and West are duly noted, as are the debates between Eastern Christianity, Judaism, and Islam. The continuous impact of Islam particularly is something that the Christian West has not had to live with for some time and so does not any longer feel its threat and challenge.

The Orthodox East saw its faith as "that which has been handed down by Christ himself and by the apostles and by the holy ecumenical councils" (p. 287). The chief idea of all Eastern theology was the idea of salvation as deification, the presupposition of which was the incarnation of the Logos of God. This deification (*theosis*) was not achievable by human power but by divine power alone, essential for which was grace and free will of man. For the East there was no conflict between Divine grace and human freedom, though the gift of *theosis* achieved by the incarnation of the Logos was a matter of divine revelation made known to mankind through sacred teaching. From a knowledge of saving history of the incarnation, death, and resurrection of Christ one could reach a contemplation and ultimately mystical union with God. Faith was absolutely essential for salvation, and this faith was attached to the orthodox dogmas of the Church through which hope and love were also possible. As divinely revealed truth, these dogmas of the Church were believed to be changeless.

The Spirit of Eastern Christendom (600–1700) does not claim to be original or startling in its content or approach. Rather it is a fundamentally meticulous historical review of Eastern Christendom that fully exploits the bibliographic sources available to Western scholars and puts the role of Eastern Christianity in proper historical perspective. It is thus a work that deserves careful examination by every student of the history of Christianity and, indeed, even by every student of East-West relations for a much fuller understanding of the contemporary religious and cultural situation in a very large part of this world.

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Dumbarton Oaks Papers. Number 27. Washington: Dumbarton Oaks Center for Byzantine Studies, Trustees for Harvard University; distrib. by J. J. Augustin, Publisher, Locust Valley, N.Y. 1973. Pp. xvii, 339. \$20.00.

This issue is predominantly art-historical, as was the 1972 Dumbarton Oaks Symposium, of which most of the lectures are included here. Since this symposium was arranged as a forum where nine young medieval scholars presented their recent research, presumably one could draw some conclusions about the future bias of Byzantine studies. The book is also significant as it presents the empirical nature of current art history, which emphasizes the archeological nature of the collection rather than, or perhaps in reaction to, theoretical speculations.

Some of this research can be noticed here. Penelope C. Mayo explains the representation of trees in the Encyclopaedia of Lambert, the *Liber Floridus* of 1120. By patient reference to the text these may be understood as symbolizing the aspirations of a Western Christian after the success of the First Crusade. G. P. Majeska deduces the late Byzantine public's view of St. Sophia from fourteenth- and fifteenth-century Russian pilgrim accounts: the pilgrim saw the interior not as a unified space enveloping the liturgical ceremony, but as an accumulation of famous relics enshrined for private devotions around the walls of the aisles. T. F. Mathews dates the Panagia Kamariotissa (a small church on the island of Heybeliada, or Chalki, about thirty kilometers offshore from Istanbul) to the late eleventh century in preference to the fifteenth century on the basis of its typical middle Byzantine recessed-brick masonry. The church is quatrefoil in plan with the transition to a dome by means of corner squinches; this type of structure, however modest in size, is significant for the interpretation of the role of Constantinople at this period. Mathews and Cyril Mango, in a postscript, discuss the implications: both suspect an influence from Armenia, and Mango suggests a connection with the Church of Christ of the Chalke Gate, designed by the Armenian emperor John Tzimiskes in ca. 972, and destroyed in 1804. Mathew's date is only an approximation, the church could have been built earlier; another possibility is that the Armenia type might itself be derived from some early Byzantine model in Constantinople. Both scholars find in the Kamariotissa a confirmation that the group of churches in Greece of the Hosios Leukas type were derived from models in the capital. Hosios Leukas has recently, but unsatisfactorily, been attributed to ca. 1011 by Ma-

nolis Chatzidakis, but Mango suggests that St. George of Mangana (1042–55) was the innovatory church. E. Kitzinger suggests that the Samson cycle on the early Byzantine floor mosaic found at Misis, Turkey (Mopsuestia) was derived from a fourth- or fifth-century parchment scroll of a picture frieze in the same format as the tenth-century Joshua Roll. He accepts two implications of his interpretation: that floor mosaicists in this period used manuscripts as one source of models and that the format of the Joshua Roll was not a medieval invention but had had antique forerunners. Margaret Frazer defines the iconography of middle Byzantine bronze doors in Italy as the Doors of Heaven on Earth. The miniatures of the Rabbula Gospels are proven by D. H. Wright to belong physically with the text of 586, produced at the monastery of Beth Zagba, located somewhere in Syria. The odd position of the Selection of Matthias and the Dedication miniature seems to be an unintentional workshop blunder. Mango and I. Ševčenko report field work in Bithynia, where their sites include a group of four-column domed churches seemingly datable to around 800; these suggest a reconsideration of the date and place of origin of the cross-in-square plan. E. J. W. Hawkins and Marlia C. Mundell describe the aniconic mosaics of Mar Gabriel in southeast Turkey and publish a fragmentary dedication inscription in Greek, which would seem to establish 512 as the date of the decoration of this transverse basilica.

W. E. Kleinbauer, on the other hand, passes beyond archeology into the interpretation of the aisled Tetraconch church plan and challenges some of the ideas in André Grabar's *Martyrium* (1946). The polemic at times underestimates the subtlety of Grabar's position, who is somewhat unreasonably criticized for statements made on the basis of the information available some thirty years ago. Kleinbauer suggests that the architectural type of this group can be explained by their primary function as cathedrals and criticizes Grabar for believing their primary function to be martyria. But their form can hardly be given a total explanation in terms of function (not all cathedrals are aisled Tetraconches!), and Grabar's thesis is broader than it is characterized, for his argument is that in the East at this period assembly churches adopted the vaults and domes of martyria chapels and that the "types" were assimilated. According to this interpretation, to ask the primary function of a church does not explain its form. Kleinbauer's idea that the Great Church at Antioch was the model for his group is also unconvincing; consequently he does not appear to have solved the problem of the origin of the type or the

reason for its use any more cogently than has Grabar.

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MODERN EUROPE

JOHN R. GILLIS. *Youth and History: Tradition and Change in European Age Relations, 1770–Present*. (Studies in Social Discontinuity.) New York: Academic Press. 1974. Pp. xiv, 232. \$12.50.

How does one study "childhood," "adolescence," "youth," "adulthood," and "old age" historically? Social historians and psychohistorians have begun to formulate approaches to the study of a stage of life as encompassing not merely an age group but a variety of interrelated psychological, social, and cultural phenomena that characterize individuals and groups at certain stages of life. The complexity of these historical processes is clearly evident, even if one were to examine a stage of life only at one point in time, and for one social class. The task becomes enormous when one tries to explore the changing conditions of a particular stage of life among different social classes over three centuries. Gillis should be congratulated, therefore, for this ambitious attempt to analyze the emergence of youth as a distinct age group in England and Germany from the seventeenth century to the present.

The study tries to relate the changing status of young people in their family and in society to demographic and social change: "If the history of youth is to be written it must focus on that interface where the expectations of the young and those of their elders interact in a dynamic manner." In doing so, he tries to explore the historical experiences from the vantage point of youth themselves, rather than merely from the perspective of the caretakers and the educators. This approach approximates a closer interrelationship between social and intellectual history than studies based on proscriptive and advice literature only.

Gillis's historical model encompasses the evolution of youth through several stages, which generally correspond to the standard periodization of overall social change from preindustrial to industrial society. He sees the historical transition from the semidependency of youth on familial and surrogate familial arrangements to the emergence of adolescence as a middle-class phenomenon in the late nineteenth century, followed by the triumph of conformity in the "era of adolescence" in the period 1900 to 1950. In this last era, where the general tendency is toward the "convergence of the life

cycles of different social groups," alternative forms of youth organization are characterized as deviant and delinquent. In weaving this pattern, Gillis compares the experience of the middle class with that of the working class and analyzes the changing functions of fraternal and peer groups in different time periods.

The author uses modernization as the general explanation for the historical changes in the status of youth. In the absence of a rigorous definition, "modernization" appears to be a catchall for the overall changes associated with urbanization and industrialization. Despite the fact that current historical research on changes in family and demographic behavior and of stages of the life cycle is pointing to a periodization that is different from the traditional historiographic divisions, Gillis assigns the development of youth into the traditional "period" slots. For example, he interprets the efforts at social control and the regulation of delinquents as the dialectical counterpart of the idealization of adolescence, from the 1870s on. This does not explain, however, the increased activity toward the reformation and control of youth as the "dangerous classes" from mid-nineteenth century on.

Another theme that is not consistently maintained in the book is the relationship between youth and the family. While Gillis pays attention to the relationship between familial change and the role of young people in the earlier periods, the theme is obscured in the later chapters of the book. One loses, therefore, a sense of the relationship between youth as a stage of the individual life cycle and between its place in the family cycle.

The lack of continuity in the volume is partly dictated by the unevenness of materials utilized by the author; the first half of the book, covering the period prior to 1870, is almost entirely based on secondary literature in which various demographic studies, as well as the work of E. P. Thompson, Natalie Davis, and Michael Anderson are extensively featured. The second half of the book utilizes primary sources and supplements them with recent writings on youth conflict. While the first half of the volume examines youth within a family and societal context, the section covering the period since 1870 focuses primarily on the study of youth groups, youth movements, and social control movements directed toward youth.

From a conceptual point of view, the author faces the difficulty of differentiating between youth as an age group and youth as a stage of life. The former pertains to age cohorts in specific time periods, in the manner in which Norman Ryder has conceptualized it in demography. The latter, on the other hand, entails an integrated analysis of the social, psychological, and socioeconomic con-

ditions of different stages in the life cycle. The challenge to the historian is twofold: one, to understand how the definitions, conceptions, and experiences of specific stages of human development were discovered and changed over time; two, to examine the changing experience of specific age cohorts or generations as they go through that stage. Historians will come closer to understanding the relationship between individual and group identity and historical change only if the interaction between these two is constantly taken into account.

The study of a specific age group such as youth is particularly valuable in illuminating the interaction between "individual time," "family time," and "historical time." On the other hand, the study of such a specific age group or stage of development must take into account the experiences of other stages of life. The transition from childhood to adolescence is not determined merely by the societal definition of adolescence, but also by the expectations from adulthood and old age in that particular society.

While Gillis offers his pioneer work as a model for the study of other stages of life, historians, working in this area, will inevitably have to revise this model and experiment with alternative models in this very difficult undertaking.

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WILLIAM E. WILKIE. *The Cardinal Protectors of England: Rome and the Tudors before the Reformation*. New York: Cambridge University Press. 1974. Pp. viii, 262. \$14.50.

The main shortcoming of this monograph, like so many doctoral dissertations, is that it is one historian writing to another, presupposing the reader's detailed knowledge of the period with little effort at interpretation or relation to other works in the field. The author impressively, although myopically, documents the neglected role of several Italian cardinals, primarily Lorenzo Campeggio, in the attempted annulment of Henry VIII's marriage. He is at pains to indicate that, since most of these cardinals were also "protectors" of the Empire as well as of England, Henry VIII and Wolsey could have their way only if they maintained a firm alliance with the Holy Roman Empire and the papacy. Once it was broken by Clement VII, the annulment was impossible, and Campeggio was finished.

Another fault of this book is the almost exclusive reliance upon British editions of sources and secondary works. For some reason the author chose to ignore significant North American scholarship. Also, the conclusion is weak, and one looks in vain

for contributions the book makes to the studies of Albert Frederick Pollard, Garrett Mattingly, or Philip Hughes. The picture of the general diocesan situation in England and corresponding chaos in Ireland is good, but one would like to see more on the structure, procedure, and abuses current at the time. When and how, for example, did the practice of consecrating those in minor orders as bishops arise?

What emerges is a good documentation of why the changes introduced by Henry VIII were not met with more resistance. British bishops were scarcely any better than their absentee Italian colleagues acting as flunkies in the lucrative intrigue of the royal and papal courts. Clement VII appears as the model of his Avignon namesake Clement VI, who purportedly said, "My predecessors did not know how to be popes," while Campeggio's ability as a canon lawyer was constantly blunted by his concern with nepotism and money. Every bureaucracy has its scandals, and certainly those of the Renaissance papacy were some of the worst, yet after reading this book one feels that then they were done with much more style and panache than those of more recent times.

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O. V. ORLIK. *Peredovaia Rossiia i revoliutsionnaia Frantsiia (I polovina XIX v.)* [Progressive Russia and Revolutionary France in the First Half of the 19th Century]. (Akademiiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1973. Pp. 297.

This volume by Orlik, like her earlier one, *Rossiia i frantsuzskaia revoliutsiia 1830 goda* (1968), illustrates Soviet interest in French history, which has grown during the two decades since Stalin's death. Orlik is a member of the French history group in the Institute of General History (formerly the Institute of History), which began publishing an annual volume of essays on French history in 1959, the same year in which the *Cahiers du monde russe et soviétique* was founded.

The main themes of this book are that the revolutions of 1830 and 1848 in France were part of a larger historical process which led inevitably to 1917, and that the relationships between French revolutionaries and Russian "progressives" during the first half of the nineteenth century were close and mutually rewarding. Thus, a number of Russians played an important role in the French revolutions, and these two French revolutions were significant in the formation of Russian revolutionary thought. Similarly, these relationships helped to increase French interest in Russia and to create

friendly relationships among the leading progressive intellectuals of the two countries. Lenin, of course, was fully aware of these developments. Indeed, eight of the first twelve footnotes cite Lenin, and one cites Marx.

Orlik supports the main themes with substantial information from some of the memoir literature of these years, the principal secondary studies published in French and Russian, and Soviet archives, especially the reports of the Russian ambassadors in Paris and of the Third Section. She cites only two French journals and apparently carried on no research in France. She defined her subject so narrowly that the substantial literature in Western languages, particularly Italian, German, and English, on Russian intellectual and social history and on intellectual relations between Russia and the West is not mentioned even in the introduction, which is a kind of bibliographical essay. Above all, the volume fails because it so narrows its analysis, presumably for political reasons, that the revolutions outside of France in 1830 and 1848 are not mentioned. Thus, she neglects the Polish revolt of 1831 and the enormous impact this had on the French view of Russia. Adam Mickiewicz, who exerted such an important role in France in the 1840s, is referred to as a "well-known poet" who knew and admired Pushkin. Soviet scholarship must indeed break out of the national framework.

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ROGER BULLEN. *Palmerston, Guizot and the Collapse of the Entente Cordiale*. (University of London Historical Studies, 36.) [London:] University of London, the Athlone Press; distrib. by Humanities House, Atlantic Highlands, N.J. 1974. Pp. ix, 352. \$20.50.

Roger Bullen's study provides historians with a detailed analysis of Anglo-French relations during the period preceding the revolution of 1848. Centering his focus upon Palmerston's role in the formation of British diplomacy, the author devotes his first chapters to a discussion of the liberal alliance, which bound England and France in the 1830s, and the "entente cordiale," which re-aligned them from 1841 to 1846. On proceeding, he then examines in the major portion of his work those problems that brought about the rupture of the entente after 1846. Bullen is especially adept in unweaving the complicated diplomatic issues surrounding the Spanish marriages crisis and in explaining their effect upon the estrangement that developed between Paris and London. His extended treatment of the Iberian question enables him to amplify and revise previous accounts of the collapse of the entente cordiale.

Despite the author's achievements, several aspects of his work are open to criticism. He could have enriched his analysis by making greater use of such source material as the press and parliamentary debates and by making a more concerted effort to measure public opinion, which had such a profound effect upon British and French diplomacy during this period. Besides these general shortcomings, certain specific weaknesses are apparent in the first part of Bullen's book. The chapters covering the period prior to 1846 deal primarily with British diplomacy and provide no deep or original analysis of French foreign policy. This deficiency probably results from the fact that the author documented these preliminary chapters with material derived from the private correspondence of numerous, largely British, diplomats and statesmen, without correcting or correlating these sources with information drawn from diplomatic archives. Bullen's assumption that the diplomatic record can be neglected because diplomats of the time confided their innermost thoughts to their private correspondence is unfounded, for French diplomatic dispatches in particular are rich in insights and information on the period from 1830 to 1846. The incomplete documentation of this first section, moreover, undoubtedly accounts for some serious errors and misconceptions, such as the author's belief that under the July Monarchy treaties required ratification by the French chambers. Fortunately though, the imperfections of this work are limited largely to the introductory sections; the main portion of Bullen's study is solid and thorough, if somewhat prolix and pedantic. Its drawbacks aside, this book should be consulted by anyone interested in understanding Spanish marriages diplomacy and the breakdown of the Anglo-French entente between 1846 and 1848.

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DAVID CHILDS. *Marx and the Marxists: An Outline of Practice and Theory*. New York: Barnes and Noble. 1973. Pp. 367. \$11.50.

V. F. PUSTARNAKOV. "Kapital" K. Marksa i filosofskaya mysl' v Rossii (Konets XIX-nachalo XX v.) [K. Marx's "Capital" and Philosophical Thought in Russia (End of the 19th-Beginning of the 20th Century)]. (Akademiiia Nauk SSSR, Institut Filosofii.) Moscow: Izdatel'stvo "Nauka." 1974. Pp. 382.

RICHARD N. HUNT. *The Political Ideas of Marx and Engels*. Volume 1, *Marxism and Totalitarian Democracy, 1818-1850*. [Pittsburgh:] University of Pittsburgh Press. 1974. Pp. xiv, 363. \$11.95.

There is no shortage of books on Marx and Engels or the movements that they inspired. Yet the out-

put of new works, both Western and Soviet, continues unabated. David Childs of Nottingham University has produced still another primer of Marxist thought and its impact throughout the world during the past century. A popular survey, it is designed for the uninitiated student and the general reader. The author is at his best when treating developments in Germany following the Second World War, an area in which he has previously specialized. The remainder, however, is disappointing. There are few fresh interpretations, the style is undistinguished, and the information is sometimes inaccurate. The bibliography, moreover, is of limited value, containing only works in English.

Pustarnakov, by contrast, has written a monographic study of a narrower subject. Sponsored by the Institute of Philosophy of the Soviet Academy of Sciences, it focuses on the impact of Marx's *Capital* in Russia during the late nineteenth and early twentieth centuries. *Capital*, interestingly enough, was translated into Russian before any other language, the first volume appearing legally in St. Petersburg in 1872. The censors considered it too dull and academic to be subversive; few, they thought, would read the work, let alone understand it. To their surprise and consternation, however, its appearance provoked excited discussion in Russian intellectual circles, and during the first four months more than one thousand copies were sold. The book was read and admired by many who ultimately rejected its author's philosophy, influencing a wide range of social thinkers from anarchists and Populists to liberals and even conservatives. Pustarnakov examines the varying interpretations of *Capital* among these disparate schools of thought, with special attention to the Populists (including N. F. Danielson, who translated the book into Russian) and Legal Marxists as well as Plekhanov, Lenin, and A. A. Bogdanov, a theorist whose ideas remain inadequately explored and understood. Pustarnakov has produced a useful study based on a careful reading of the Russian sources, though he ignores the sizable body of Western scholarship on the subject.

In the third book under review, Richard Hunt of the University of Pittsburgh combines an analysis of the early writings of Marx and Engels with an account of their lives up to 1850—a second volume will deal with their later years. The result is a valuable work of biography as well as of intellectual history. Hunt finds that—in their younger years at least—Marx and Engels were neither totalitarians nor reformers but rather democratic revolutionaries who rejected both terrorism and dictatorship, proletarian or otherwise. What they envisioned, he says, was a spontaneous revolution carried out by the people themselves and ush-

ering in a direct or "participatory" democracy without professional leaders or administrators. Hunt's analysis of the intellectual development of Marx and Engels is closely reasoned and amply documented, though he tends to soft-pedal, if not to explain away, the more authoritarian aspects of their personalities and doctrines, which begin already to emerge during this formative period. His book is nevertheless one of the most lucid and provocative works on Marxism to have appeared in recent years, and one looks forward eagerly to the concluding installment.

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ISTVÁN DIÓSZEGI. *Österreich-Ungarn und der französisch-preussische Krieg, 1870–1871*. Budapest: Akadémiai Kiadó. 1974. Pp. 311.

The Franco-Prussian War of 1870 brought not only disaster to the Second Empire, but diplomatic defeat and a year of internal crisis in Austria-Hungary. Diószegi's account, which appeared for the first time in Magyar in 1965, differs from the usual monograph in diplomatic history; there is much more description of the forces operating on diplomacy than there is on its conduct. One merit of this emphasis is that it unmistakably brings out the interaction between internal and external politics. Diószegi's account of the shift of the Hungarian Deákists from an anti-Prussian to a mildly pro-Prussian stance in about a year's time is especially striking. This shift, he shows, stemmed from fears of aggressive "Slavdom," which were activated largely by the Russian government's denunciation of the Black Sea clauses. Absurd perhaps, but Diószegi's analysis of the Budapest press and of other evidence pretty well substantiates it. His account strengthens the conclusion that the foreign policy of Austria-Hungary was sometimes decided in Budapest rather than in Vienna. It is unfortunate, however, that Diószegi does not focus his attention on other issues related to his theme. For example, there is some discussion of Italian policy at the beginning of the war, but no mention of the repercussions in Austria of the occupation of Rome. There is, moreover, nothing at all on the fiscal and economic condition of the monarchy in 1870, an odd omission, given the place of publication. Diószegi does point out that the army required about six weeks to mobilize, but offers the reader no explanation as to why. Very clearly Beust and Francis Joseph were leading from weakness, but the reasons could be made clearer.

Despite the generally sound and refreshing interpretation there are a few slips. For example, Diószegi is convinced—like the Magyar chauvinists of a

century ago—that the tsarist government, imbued with pan-Slavism, was determined to have Constantinople (p. 12), that everyone in the Austrian court party was bent on revenge on Prussia after 1866 (p. 15), that a French "people's war" might have beaten the Prussians in the autumn of 1870 (pp. 118, 221), and that the French were imbued with the determination to revenge themselves on Germany after 1871 (p. 245). But Diószegi's skillful use of the archival material in Dresden, Potsdam, Merseburg, and especially Budapest makes this a valuable study. His portrayal of Beust, driven by the Hungarian hotspurs into a foolish policy, is sound. It is grist, too, for the mills of those who believe that diplomacy, even in autocratic states, does not operate in a vacuum but rather is subject to powerful, usually conflicting internal forces. This is one reason why foreign policy is not always rational but sometimes rather absurd, sometimes tragically absurd.

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DWIGHT E. LEE. *Europe's Crucial Years: The Diplomatic Background of World War I, 1902–1914*. Hanover, N.H.: University Press of New England, for Clark University Press. 1974. Pp. xiv, 482. \$15.00.

So complex are the years 1902–14 that most scholars of diplomacy investigate only particular aspects of the age. While few historians have the courage to tackle it as a whole, Professor Lee, after twenty years of inquiry, has done just that. He offers here an excellent, up-to-date, and much needed synthesis of the entire "crisis period."

Lee's intention is not to convince us of one nation's guilt in causing World War I, but rather to demonstrate that "events and decisions led, often unwittingly, to a situation in which the only available recourse was war." His concern is with a detailed examination of diplomatic and political affairs aimed at an "overall approach to the question of why the war of 1914." Essentially dividing his book into two segments, he first provides an analysis of the diplomatic revolution accomplished by 1907. Careful consideration is given to the successful, if partial, separation of Italy by France and Britain from the Triple Alliance with a resultant change in European alignments, the emergence of a dynamic foreign policy under the direction of Theophile Delcassé, Germany's diplomatic failure to break the Entente during the first Moroccan crisis, and the shaping of the Anglo-Russian convention of 1907. It is underscored that in spite of Anglo-French-Russian arrangements that existed by 1907, a genuine Triple Entente was not yet achieved, German protestations of encirclement

notwithstanding. It emerged only after tempering in the fires of the Bosnian and Agadir crises, the Balkan wars of 1912-13, and Sarajevo. A description of these events constitutes Lee's second segment.

Certain interpretations emerge as striking, namely that the Bosnian crisis was a "dress rehearsal" for July 1914 with the powers taking the same position in both cases and for the same reasons and that while Agadir proved a watershed, its aftermath having produced two camps, the lines between states did not become so tight as to preclude interpenetration. This last view is most interestingly supported by a discussion of German-British cooperation in defusing the Balkan wars. The subsequent delineation of Sarajevo and events leading to general conflict are objectively handled in depth. Lee concludes that if Austria, backed by Germany, took the first step toward conflagration, Russia took the second. Neither Germany nor England proved strong enough to deter their respective allies. While agreeing that the Austrian and German governments took a gamble, the author rejects Fritz Fischer's thesis of a German preventive war. War came, says Lee, because each of the major states acted out of a sense of desperation in maintaining its own interests.

This study takes advantage of both the best secondary literature available and all published documentary collections that are germane. Supplementary materials, accessible now for some time in various European archives, however, have apparently not been incorporated. While the footnoting is sometimes spotty and minor printer's errors are in evidence, the bibliographical essay should prove enlightening for advanced students and professionals alike.

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A. A. SOLOV'EV *et al.*, editors. *Russkie internatsionalisty v bor'be za Vengerskuiu Sovetskuiu Respubliku 1919 g.: Sbornik dokumentov* [Russian Internationalists in the Struggle for the Hungarian Soviet Republic, 1919: A Collection of Documents]. (Instytut Marksizma-Leninizma pri TsK KPSS, Glavnoe Arkhinoe Upravlenie pri Sovete Ministrov SSSR, Instytut Istorii Partii pri TsK VSRP, Vostochno-istoricheskii Institut i Muzei VNA.) Moscow: Izdatel'stvo Politicheskoi Literatury. 1972. Pp. 251.

This collection of 176 documents is a modest testimonial to what is known in Communist circles as "proletarian internationalism." Like other Soviet contributions on this subject, it records the interest displayed by the Bolsheviks in nascent Communist groups and parties during and after the First World War and the aid they gave to them. Of

these the Hungarian Communist party was the most successful in the short run.

Solov'ev's volume contains numerous extracts from the Soviet press devoted to events in Hungary in 1919 expressing sympathy for Bela Kun's regime. To these are added previously unpublished documents from Soviet and Hungarian archives, including the Institute of Marxism-Leninism in Moscow and the Institute of Party History of the Hungarian Socialist Workers' Party in Budapest. These contain reminiscences of and references to Russian and Ukrainian prisoners of war and deportees who were interned in the Hungarian area of the Dual Monarchy. The downfall of the Central Powers led to their liberation and involvement in the Hungarian Communist movement. They were sufficiently numerous to form a Russian battalion that fought as part of the Hungarian Red Army against the Romanians in 1919.

To students of Bolshevism the most interesting documents are those that throw additional light on the efforts of the Bolsheviks in exile to spread their views among Russian prisoners of war during the Great War. With the tacit blessing of the Habsburg authorities, fairly large quantities of printed material reached the prisoners of war from Switzerland. The tenuous contacts Lenin and his associates maintained with their supporters and sympathizers in the camps contributed to the emergence of a section of the Bolshevik party in Budapest in December 1918. Its members published a newspaper in Russian and did their best to rally support for the Soviet cause.

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FRANÇOIS CROUZET. *Le conflit de Chypre, 1946-1959*. In two volumes. (Centre Européen de la Dotation Carnegie pour la Paix Internationale. Études de cas de conflits internationaux, 4.) Brussels: Établissements Émile Bruylant. 1973. Pp. 478; 482-1187, 1 table, 1 map. 3,040 fr. B.

STEPHEN G. XYDIS. *Cyprus: Reluctant Republic*. (Near and Middle East Monographs, 11.) The Hague: Mouton; distrib. by Humanities Press, Atlantic Highlands, N.J. 1973. Pp. 553. \$37.00.

There are two fundamental factors about Cyprus. One is geography, an island off the southern mainland of Anatolia, from which fact derives Turkey's strategic interest in it, especially in view of the possession of the Aegean Islands by Greece. The other is its ethnic composition—four-fifths Greek, one-fifth Turkish. Cyprus has never been an independent state—not at least until 1960—and there is no such thing as a Cypriot nationality. In the eyes of the Cypriot Greeks and of Greece, *enosis* is a simple case of national irredentism.

One hundred years ago Britain obtained control of the island, on a supposedly temporary basis, as a base from which to help Turkey resist Russian aggression. More recently, with the British imperial retreat and other Middle Eastern developments, the United States has acquired a stake in the status of Cyprus. The fundamental reason for the American interest is very similar to the British a century ago. These are the elements that go into the making of the Cyprus problem at the present time.

Professor Crouzet's essentially collective work traces the vicissitudes of the problem from the end of the Second World War until the compromise settlement of 1959, which could find no better solution than the creation of an independent state. It was a fragile solution, as subsequent events have shown, but represented the best equilibrium that could be achieved among the antagonistic forces that centered on the island. In the initial phase the issue was one between the Greek Cypriots and Britain. The contest proved to be another illustration of the ineffectualness of regular armed forces and normal legal procedures in dealing with insurrection that resorts to guerrilla tactics and terrorism, tools of which Colonel Grivas made skillful use.

After a while, and especially in the wake of the Suez fiasco of 1956, Britain was willing to shift her requirement from Cyprus as a base to bases in Cyprus. But this British willingness to retreat introduced the Turkish factor, for Turkey, which had been content with the British presence, interposed an immovable veto on the possibility of *enosis*. Once on his side, Archbishop Makarios had abandoned that hope, Turkey became willing to consider the compromise of Cypriot independence. Direct Greco-Turkish negotiations were quickly concluded in Zurich, then underwritten in London, Britain retaining the sovereignty of two bases.

This is the story that Crouzet relates at great length and in sometimes wearying detail, but unquestionably with exhaustive thoroughness. Although he stresses Britain's responsibility, his judgments are balanced, full of the "on the one hand, but on the other" approach. Like other studies in the same series (the Saar, Morocco, Trieste), sponsored by the Carnegie Endowment for International Peace, this is a valuable case study that has larger implications than those of the limited locale with which it deals.

Professor Xydis is as qualified an authority as is to be found in the United States on the score of Greek external affairs. His book fills out and supplements Crouzet's study, dealing in exhaustive fashion with the last phase of the settlement only, the period from 1958 to 1960, from the point when the shift in the British, Greek, and Turkish posi-

tions made possible the elaboration of the final compromise of Cypriot independence.

The direct approach of the Turkish foreign minister to his Greek counterpart, in connection with a session of the United Nations in December 1958, was the turning point. The latter's sound observation that the Cyprus problem had so far been dealt with in the context of *petite politique*, when that of *grande politique* should have been the case, unhinged the stalemate of hitherto irreconcilable requirements.

Though, admittedly, there are gaps in the currently available documentation, the Turkish especially, that both writers note, it is unlikely that additional disclosures will contribute substantially to a modification of the tale unfolded in these two works. Both writers approach their task from the standpoint of considering the Cyprus problem as an instance, or case study, in conflict resolution. The changing context of international relations adds relevance to such a point of view; it also illustrates the limitations of the type of results it is possible to achieve. In the particular case of Cyprus, Xydis' closing comment is most apt, that subsequent events have shown to be prescient: "Old vinegar, it seemed, had been put into new bottles."

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MERVYN JAMES. *Family, Lineage, and Civil Society: A Study of Society, Politics, and Mentality in the Durham Region, 1500-1640*. New York: Oxford University Press. 1974. Pp. x, 233. \$16.00.

Understanding the nature of society in the counties is essential to the comprehension of developments in England in the century and a half before the Civil War. Mervyn James has attempted an ambitious analysis of the Durham region in that period, and even if he does not wholly succeed in his effort at "total history," he still provides much useful information that illuminates national as well as local history. The evolution of society in the Durham region was subject to some unique influences such as the effects of the Northern Rising of 1569 and the impact of the developing coal industry, and because of this, generalization on the basis of Durham evidence is risky. Nonetheless, James has some suggestive observations to make, notably his contention that society evolved from what he calls a lineage society to a civil society. By the former term he means a society based on the great landed families and their connections, by the latter a society characterized by C. B. Macpherson's "possessive individualism." Such a transition involved a break with "the bounded horizons and particularized modes of

thought" of the traditional, inherited society, and James is surely correct in suggesting that this was one of the most significant developments of the period. Up to 1570 the older patterns are seen as dominant; after that date a new society was emerging that would see things in quite different ways.

Some of what James says is speculative. Generalizations about the nature of family organization, especially before 1570, are based on limited evidence, much of it drawn from wills, and it could be argued that this produces some distortion. The least successful sections are those that deal with the popular mentality; here the evidence is simply too scanty to allow the kind of analysis that would be desirable. Instead of receiving a fully rounded picture, one is left substantially at the level of formal religion; admittedly, James provides at that level a convincing analysis of the introduction of Protestantism in the region. The most successful sections are those which deal with county politics and the county community, and here James makes a valuable contribution to the growing body of studies on local political loyalties.

The absence of any significant body of previous studies on the area has complicated James's task. He notes, for example, the existence of an excellent series of parish registers in Durham, but since no comprehensive analysis has been made of them, his own discussion of the demography of the period is "a matter of rough estimates, round figures, and general impressions." Despite such shortcomings, this is a valuable study, and one hopes that it will inspire further detailed investigation into the implications of the transition from "medieval" to "early modern" in the North of England.

ROGER HOWELL, JR.
Bowdoin College

MARTIN FLEISHER. *Radical Reform and Political Persuasion in the Life and Writings of Thomas More*. (Travaux d'Humanisme et Renaissance, 132.) Geneva: Librairie Droz. 1973. Pp. 183.

In view of the spate of books expounding Thomas More's *Utopia* that in the past quarter century has flooded the scholarly world, that world might well be forgiven if it threw up its hands and exclaimed, "Enough! Please God, not another!" But there is another, Martin Fleisher's *Radical Reform and Political Persuasion in the Life and Writings of Thomas More*, and the scholarly world needs to pay attention to it. The title itself indicates the author's intention of extending his investigations well beyond the bounds of More's most famous book. In his prefatory acknowledgments Fleisher indicates that his work differs from that of two of his immediate predecessors "in its attempt to analyze all of

More's writing . . . [and] to understand More's thought in terms of its unifying principles." To a remarkable extent the author achieves what he ambitiously attempts. With *Utopia* as recurrent focal point, he subtly meshes More's writings and his deeds into an intricate and subtle pattern that perhaps comes closer than any previous effort to doing justice to and making sense of the complexity and simplicity of that perplexing man. There is no way in the allotted space to summarize, much less do justice to, Fleisher's intricate argument. Much of it is original, most of it is persuasive, and all of it is at least plausible, commanding serious consideration. The reviewer especially admired the section on "Dialectic and Rhetoric." There the author sets forth skillfully the old conflict between those two approaches to truth and analyzes with precision the deep moral and religious implications of More's allegiance to rhetoric.

Fleisher is aware of the point at which his study may be most vulnerable to adverse criticism. He prefaces one of his most interesting points with the clause, "It is, I believe, not perverse scholarly ingenuity [to] suggest . . ." Occasionally witnesses to the breath-taking intellectual maneuvers by means of which Fleisher seeks to make manifest the interrelation and compatibility of the "unifying principles" of More's thought may wonder whether they are in the presence of solid demonstration or verbal legerdemain. Yet those occasions are rare, and even when they occur the reader would do well to hesitate long before making an adverse judgment. On the very rare occasions when he made such a judgment, the present reviewer was inclined to do so charitably. By experience he is painfully aware of the temptation to see more in *Utopia* than a cold assessment of the evidence would suggest is there. Indeed, the reviewer suspects that it is a temptation to which he himself has more than once succumbed.

J. H. HEXTER
Yale University

A. HASSELL SMITH. *County and Court: Government and Politics in Norfolk, 1558-1603*. New York: Oxford University Press. 1974. Pp. xviii, 396. \$27.25.

This important study provides an outstanding example of the way in which the examination of a single locality can enlarge our understanding of national problems. Dr. Smith's subject is Elizabethan Norfolk, a county torn by factionalism and intense political rivalry among its leading gentry. The struggles began shortly after the execution of the chief political patron of the county, the duke of Norfolk, in 1572. His removal created a vacuum in

local politics that a number of gentlemen eager for power, prestige, and economic gain sought to fill.

By the mid-1580s these men had coalesced into two distinct groups, each holding a particular view of county government that looked beyond mere personal gain. One group, led by the irascible Sir Arthur Heveningham, comprised what Smith calls the "court" gentry. These men, who came from well-established but often financially troubled Norfolk families, sought to use the power of the central government as a means to political ascendancy in the local arena. For this reason, they welcomed the administrative innovations of the 1580s and 1590s, working zealously as deputy lieutenants, royal patentees, and commissioners for purveyance. Against them stood a group of magistrates clustered about Sir Nathaniel Bacon of Stiffkey. This faction, composed of men from families more newly risen in Norfolk society and who displayed marked puritan sympathies, represented the "county" viewpoint. They worked chiefly through quarter sessions and assizes to preserve the autonomy of their locality and their own personal interests, which they saw threatened by the activities of Heveningham and his allies.

Clashes between these groups, which were frequent and sometimes violent, saw almost every office of local government manipulated for factional ends. Smith suggests that such controversies eventually became bound up in national politics, with Parliament serving as the final arbiter of questions that had gone unresolved at the local level. Indeed, Norfolk's experiences indicate that "the political issues of the early seventeenth century did not originate in parliament by some process of self-generation and then filter down to the county; rather they had their origins in county communities which regarded statute as the ultimate solution of their constitutional, social, and economic problems."

Smith has written an excellent book that deserves to be read by every student of Tudor-Stuart politics. What he has done for Elizabethan Norfolk cannot help but become a model for similar investigations of other county communities in this period.

G. L. OWENS
Huron College,
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W. SPEED HILL, editor, *Studies in Richard Hooker: Essays Preliminary to an Edition of His Works*. (The Folger Library Edition of the Works of Richard Hooker.) Cleveland: Press of Case Western Reserve University, 1972. Pp. xiv, 363. \$12.50.

In 1836 John Keble's edition of Hooker's works was published; in 1888 the recension by R. W.

Church and Francis Paget appeared; in 1970 the decision was made to publish the Folger Library Edition of the Works of Richard Hooker. W. Speed Hill, as general editor, issued his *Richard Hooker: A Descriptive Bibliography of the Early Editions, 1593-1724* (1970) as a trial edition, which when revised will appear in the complete works. Now Dr. Hill has edited six preliminary studies.

This volume is a collection of six essays, together with a well-selected annotated bibliography of forty-two pages. The longest essay (seventy-four pages), by Dr. W. D. J. Cargill Thompson of King's College, London, discusses "The Philosopher of the 'Politic Society': Richard Hooker as Political Thinker." Dr. Thompson acknowledges that Hooker's reputation as a political thinker has declined, that some of his ideas were anticipated by the writings of Marsilius of Padua, George Buchanan, Stephen Gardiner, and John Whitgift. Nevertheless, Hooker as an apologist for the Church of England and the status quo, as a polemicist writing against Walter Travers and Thomas Cartwright, and as a thoughtful exponent of the two eternal laws and subdivisions is deserving of study.

Harry C. Porter of the University of Cambridge presents the essay "Hooker, the Tudor Constitution, and the *Via Media*." He helps the reader to put Hooker in a proper context, and he provides the background by emphasizing the new learning of the 1530s, by stressing the Erasmian humanist tradition, and by studying the ideas of Cranmer, Hooper, Ridley, Parker, Cartwright, and Whitgift. W. Speed Hill examines the evolution of Hooker's *Laws of Ecclesiastical Polity*. He stresses the importance of Walter Travers's attack as an initial cause for Hooker's book and argues for the role of Richard Bancroft, Sir Edwin Sandys, and George Cranmer in making the *Ecclesiastical Polity* a quasi-official apologia. Egil Grisliis presents Hooker as a theologian concerned with principles. After reviewing different approaches to Hooker, he examines with Hooker the Puritans' claims and finds them wanting. He then presents Hooker as interpreter, as one who strove more valiantly for objectivity, sound knowledge, careful writing, charity, and for the idea of revelation joined with judicious reason.

John E. Booty, who has given us the best edition of John Jewel's *Apology of the Church of England* (1963), and also his study, *John Jewel as Apologist of the Church of England* (1963), indicates in his essay, "Hooker and Anglicanism," that Hooker emphasized the importance of reason, free will, church, tradition, and human nature. Hooker reflected the Anglicanism of the sixteenth century and influenced the development and direction of this system in the seventeenth century. Finally, in a fascinating essay, Georges Edelen analyzes "Hooker's

Style." He indicates that Hooker utilizes irony, epigrams, contrasts, and logical subordination; also he reveals that Hooker is open to the charge of being discursive, abstract, archaic, unclear in his referents, and of piling too many elements of thought before his main clause.

This book is remarkably clear of errors. I found only two misprints or misspelled words. Perhaps it is worth mentioning that Travers's *Ecclesiasticae Disciplinae* was printed by Michael Schirat at Heidelberg, not at La Rochelle or "Rupellae" as given in the book's imprint.

LELAND H. CARLSON
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COLIN G. C. TITE. *Impeachment and Parliamentary Judicature in Early Stuart England*. (University of London Historical Studies, 37.) [London:] University of London, the Athlone Press; distrib. by Humanities Press, Atlantic Highlands, N.J. 1974. Pp. xi, 249. \$15.00.

This book is about the "revival of impeachment" in the Jacobean Parliaments. One of its contributions is to emphasize the need for quotation marks around the phrase. Impeachment finally came to signify the one lawful criminal process in Parliament (for attainders are rather a substitute for process), consisting of specified accusation by the Commons and trial, adjudication, and punitive decision by the Lords. There was no such definite thing lurking in medieval parliamentary practice to be revived, nor did the Jacobean Parliaments invent it supposing they were reviving the past. The final product was a possibility among other related ones suggested by the medieval records; it remained a possibility, some steps along the way to selection as the viable one, when James I's parliamentarians had done their work. Prompted by the precedents and by their own preferences and the exigencies of each case, those parliamentarians experimented with various means to the approximate end of impeachment, while consciously achieving the revival of something vaguer than even the variegated cluster of procedures serving that end—the "judicature" of Parliament.

The book is essentially devoted to making these points in careful detail. Its focus is procedural, but within that focus the narrative of the several impeachment cases is clearly presented. In explaining the obsession with judicature and why the cases were taken up and pursued in the forms adopted, the author seems to me a bit shy of making hypotheses and of reaching for the full flavor of a disastrous and creative political environment, but he certainly warns effectively against explanatory simplism. The book speaks primarily to a technical interest in early seventeenth-century his-

tory. That is to say, it is not directly in point for contemporary American concern about what the impeachment process has been and might be. Less directly, however, it is worth the consideration of people with that concern. The lesson I would draw in such larger terms—for which the author of course bears no responsibility—is simply that impeachment in its ultimate English form, which passed with modifications into our Constitution, did not have to be. The seventeenth-century quest for parliamentary judicature could have led to a more profound and less fortunate muddying of the distinction between legislative and judicial offices than the somewhat residuary impeachment process finally came to represent. On the other hand, without the strange contingencies of Jacobean politics, impeachment might never have been launched toward its consummation as a pillar of two constitutions. At a time of somewhat heady popular conviction that the impeachment process "works," it might be well to reflect whether we would be much worse off without it.

CHARLES M. GRAY
Yale University

J. S. MORRILL. *Cheshire 1630–1660: County Government and Society during the English Revolution*. (Oxford Historical Monographs.) New York: Oxford University Press. 1974. Pp. x, 357. \$17.75.

This book is a notable addition to the important body of studies on the English counties during the Civil War inaugurated by A. M. Everitt's *The Community of Kent and the Great Rebellion* (1966). Prefaced by a brief but useful survey of Cheshire's social and political circumstances in the prewar decades, the book gives us a detailed political and administrative history of the county from 1640 to the Restoration.

It drives home once again the astonishing variety of English regional life in the seventeenth century. The useful model Everitt offered for Kent has to be modified extensively when applied to Cheshire. Background circumstances were, of course, different. The northern county was a small compact society, remote from the capital, and ruled by a stable and long-established elite of local gentry. They resented government policies in the 1630s but had raised vocal opposition only on the eve of the meeting of the Long Parliament. They entered the war with the greatest reluctance, and only a few had deep commitments to either of the contending parties. The shire was the site of military operations throughout the war; Chester fell to Parliament only in January 1646.

Quite in contrast to the situation elsewhere, the Parliamentary in Cheshire was dominated almost totally by one man, the local military commander,

Sir William Brereton, a middling squire of ancient lineage and an outspoken prewar critic of Charles I's regime. His withdrawal to the national political scene after 1646 loosened up the local political situation but led to little overt antagonism. The county remained quiescent during the second Civil War; royalists were generally allowed to work their passage on easy terms. After 1649 the county, like many others, fell under the sway of a new local elite, men of lesser consequence but few of them recruited from outside the ranks of the middling gentry. Only in 1659, in Booth's Rebellion, did the older leadership re-emerge, a characteristic alliance of Royalists and Presbyterians.

The book is particularly rich in its detailed account of revolutionary finance and local administration. Morrill gives a full and illuminating picture of the economic burdens of the war, in a county where fighting went on continuously and almost all the money collected was expended locally. He is able to give a detailed account of the effect of the various parliamentary taxes levied in the county. A particularly original and important part of the book is the author's argument that the local government of Cheshire in the 1650s was more competent and more responsive to social needs than any preceding. At the same time he emphasizes the local gentry's continuing fears of social disorder—arising from different sources at different times, but mostly aroused by the radicals after 1646.

This is a well-researched and intelligently written work that offers a persuasive but flexibly structured interpretation of the Civil War years in this local setting.

WALLACE T. MACCAFFREY
Harvard University

BRIAN MANNING, editor. *Politics, Religion and the English Civil War*. New York: St. Martin's Press. 1973. Pp. viii, 272. \$13.95.

These six essays examine the role of certain categories of men and sectors of society. All concern, in varying degrees, religion and politics. Their approach, by way of local history, the history of ideas, statistical analysis, women's activities, and the relation of class consciousness to contemporary issues, brings fresh understanding of a complex story. The five authors were stimulated by research undertaken in the history department of the University of Manchester. This, together with Brian Manning's prefaces to the book as a whole, and to each individual study, lends, in spite of their range, a sense of unity to the collection.

"Puritanism and the Ecclesiastical Authorities" by R. C. Richardson describes the "case of the diocese of Chester," contrasting the leniency to-

ward its Puritans by the Elizabethan government with the insistence upon uniformity by Charles I and Archbishop Laud. Earlier appreciated as a bulwark against Catholicism, Puritans later appeared merely as a dissident group. Their resulting harassment thus became another factor in growing opposition to royal policies. Keith Lindley in "The Part Played by the Catholics" provides statistics based on the records of London and eight counties to support the conclusion that Catholics did not form any very considerable part of Cavalier forces. A few even may be discovered in the parliamentary ranks. Lindley points out that Catholics had been widely unpopular, heavily fined as recusants, and generally seem to have preferred neutrality where they could maintain it.

Patricia Higgins, carrying on the earlier research of Ellen A. McArthur, describes, in "The Reactions of Women," a number of activities—ranging from work on fortifications to petitioning on behalf of peace and the Levellers—which modify public statements about female submissiveness. Women had a wider appreciation of the political situation than often conceded, though this found some recognition in contemporary satire. "The Levellers and Christianity," by J. C. Davis, presents one of the best modern studies of the subject. The Levellers were Christian pragmatists, by no means doctrinaire, and supported the laws of necessity, of Nature and of God. They stressed the importance of "self-propriety" and believed—like some later Quakers—that there could be no toleration without civil liberty.

Two studies are by Brian Manning. "The Aristocracy and the Downfall of Charles I" admirably depicts the king's mistreatment of those who should naturally have supported him and his preferred dependence upon personal friends and courtiers. "Religion and Politics: The Godly People" deals with "the middling sort" of persons who eventually became mainstays of the parliamentary side. Tradesmen, yeomen, and artisans were often resentful of what they thought unfair assessment in taxation, of growing exclusion from a part in local government, of the authoritarianism of the Established Church, and of the ungodliness of many of the nobles and gentry. The political issues that absorbed the attention of England's upper echelon in 1640 at first scarcely affected the Puritans, strongest in the middle bracket. As they were sneered at as "roundheads," and their grievances seemed most likely to be remedied by the parliamentary side, they became involved in constitutional, as well as ecclesiastical, controversy and demands. Manning thinks that to many of these people, it appeared impossible to be both godly and gentle; beneath Puritanical pretext lay marked class consciousness. Not everyone will en-

dorse conclusions in this and the other essays, but all students of the period will find much to encourage further investigation along the lines suggested.

CAROLINE ROBBINS
Rosemont, Pennsylvania

SIR NEIL CANTLIE. *A History of the Army Medical Department*. In two volumes. Edinburgh: Churchill Livingstone; distrib. by Longman, New York. 1974. Pp. 518, 3 maps; 447, 7 maps. \$42.00 the set.

Sir Neil Cantlie, late director-general of the army medical service, has attempted to remedy the lack of a history of the medical department of the British army by producing a study of the medical service from Cromwell's New Model Army to the establishment of the Royal Army Medical Corps in 1898 based on a vast array of published and unpublished materials. Although he concentrates on the administrative development of the medical service, Cantlie also deals with the major diseases that afflicted the British army and the general treatment of war casualties.

During most of the two and a half centuries covered by this study, the British medical service consisted of two distinct branches of medical officers: the regimental branch, in which surgeons served with regiments, and the medical staff branch, which comprised garrison surgeons and administrative officers in hospitals. In the mid-eighteenth century an army medical board was established, which administered the service until it was replaced in 1809 by an army medical department supervised by a director-general. Until the Crimean War there was no rank-and-file branch in the medical service, and soldiers—usually misfits—were assigned from military units for hospital duty. Nor did the Cardwell reforms in the 1870s, which united the regimental and medical staff branches to form a single corps, enable medical officers to exercise command over the soldiers in the army hospital corps and their patients.

In the first volume, Cantlie provides a medical history of British forces during the eighteenth-century wars, with special emphasis (in seven chapters) on the French Revolutionary and Napoleonic wars and the lesser wars in Africa and Asia (1824–53). But one of the most interesting chapters in this volume is that which deals with the important work of the very able James McGrigor as director-general of the army medical department from 1815 to 1851. The second volume presents a very detailed medical history of the British army during the Crimean War. After delineating the reform of the medical service following the Crimean War, Cantlie reviews its role in the wars in Asia and Africa (1873–98) and the establishment of the Royal Army Medical Corps. He concludes the

work with an incisive discussion of army health (1836–98) and interesting essays on British military medical writers and notable medical officers. This is indeed an excellent study enhanced by very useful appendixes and maps.

J. O. BAYLEN
Georgia State University

GRAHAM MIDGLEY. *The Life of Orator Henley*. New York: Oxford University Press. 1973. Pp. ix, 297. \$19.25.

"It was worth while being a dunce" in Pope's time, Dr. Johnson commented, and plainly no accomplishment of John "Orator" Henley could outlive the recognition accorded him in books 2 and 3 of Pope's *Dunciad*. Graham Midgley has nonetheless looked behind the grotesque bust of the dunces' Poet's Corner and has found a man—grotesque still—but not unworthy the attention of a full-length biography.

The book, it must be said, takes a while to arouse one's interest. This is partly because Midgley describes in excessive detail the plan and proceedings of the "Oratory" that Henley opened for divine services, lectures, and disputes after he separated from the Established Church in 1726, and partly because some of the potentially most interesting episodes—Henley's affair with Mrs. Tolson, for example, and his quarrel with Bishop Gibson—have resisted Midgley's efforts at full recovery. But much of the rest of the book is absorbing, most notably the accounts of Henley's controversies with Pope and Christopher Smart and of the absurd disputations in the Oratory, which so frequently resulted in total disorder that by 1749 the attendance of two constables was required.

Henley was not an attractive man. A paid government informer, he at times took vengeance on those who disturbed his Oratory meetings by reporting their antigovernment statements. He had an intense anger and did not easily forgive. He was not always scrupulous in his publications or in fulfilling his obligations to his Oratory audiences, who paid for their admissions. Because of these traits, he was an abject, lonely man, rejected at last even by his wife, who left her little portion to her brother and sister rather than to him. His pulpit had protective spikes around it, a symbol of his isolation and outspokenness.

Mr. Midgley neither conceals nor excuses Henley's innumerable faults. He does, judiciously, see them in context. For the industrious and irrepressible Henley was a poet of at least modest ability and, more important, a pioneer in Church reform who might have left his mark upon the Church had he possessed the wit to divert attention from his faults to what was truly good in him. Although a

dunce, he was an extraordinary dunce, very well suited to Pope's measure.

BERTRAM H. DAVIS
Florida State University

HERBERT M. ATHERTON. *Political Prints in the Age of Hogarth: A Study of the Ideographic Representation of Politics*. New York: Oxford University Press. 1974. Pp. xx, 294, 122 plates. \$32.00.

Public opinion, which Atherton rightly calls "a much neglected subject," looms as the *eminence grise* of history, as real as it is elusive. For the seventeenth and eighteenth centuries, Interregnum sermon literature, Restoration poems on affairs of state, and now Georgian political prints, give limited but fascinating insights.

Atherton has come upon a storehouse and therefore considered—probably correctly—that certain basic chores of inventory and categorizing are necessary. At its best this exercise in ordering allows a profitable concentration on single topics or figures (party or patriot king, Excise or Jew Bill, Walpole or Bute); at its worst it begets a file-card anesthesia. We have, then, a sense of accumulation rather than assimilation, emphasized by the tactics of the book: it opens with a numbing survey of printmakers, deals disparately with circulation, propaganda, and government attitude, and equally disparately with parties and other topics; next it looks at three critical issues under "City [?] Interest," then at the graphic treatment of five major politicians (in two chapters); and it concludes with a fine overview. But couldn't the printmakers have been cataloged in an appendix, or the subsectioned chapters made more integral, or the overview placed so as to introduce what is terra incognita for most of us? Such questions are posed because this treasury of information merits optimum presentation.

The 122 illustrations have been carefully selected but are sometimes so tantalizingly small that it is impossible to read the script. Still, they are here, which is much more than we have ever had and, though known or posited dates in the captions would have helped, they nonetheless constitute a rich and useful panorama of political response. And indeed without Atherton's help, without his discerning eye, the ordinary viewer would miss much of the riches and not fully comprehend the extent of their usefulness.

Atherton's study, then, is highly evocative. Every reader will wish that the book responded to his own particular interest—which is probably the surest tribute to the essential nature of political prints and this work on them.

HOWARD H. SCHLESS
Columbia University

W. S. LEWIS *et al.*, editors. *Horace Walpole's Correspondence with Henry Seymour Conway, Lady Ailesbury, Lord and Lady Hertford, Mrs Harris; Horace Walpole's Correspondence with Henry Seymour Conway, Lady Ailesbury, Lord and Lady Hertford, Lord Beauchamp, Henrietta Seymour Conway; Horace Walpole's Correspondence with Henry Seymour Conway, Lady Ailesbury, Lord and Lady Hertford, Lord Beauchamp, Lord Henry and Lord Hugh Seymour*. (The Yale Edition of Horace Walpole's Correspondence, volumes 37-39.) New Haven: Yale University Press. 1974. Pp. lxi, 584; 578; viii, 557. \$22.50 each.

These three most recently published volumes of *Horace Walpole's Correspondence*, like their predecessor, contain Walpole's intimate correspondence with his close relatives; in this case, the descendants of his mother's sister, the Seymour Conways. Walpole's own generation of Conways included two brothers, their charming wives, and two sisters. In addition, three of the younger generation are represented briefly in these volumes.

The subject matter of the letters depended to a large extent on the careers of the two cousins and the places to which their professions took them, as well as on the character of the period covered, 1737-95. Lord Hertford represented his sovereign as ambassador to the court of Louis XV of France and as lord lieutenant of Ireland. He later served as lord chamberlain of the household. Henry Seymour Conway, clearly Walpole's favorite, had two principal careers. His first love was the army, but his independent spirit led to temporary disappointments. He gained, lost, then regained a regiment, and finally attained the rank of field marshal. He was less happy in politics, but accepted the appointment as secretary of state in 1765 and undertook the leadership of the House of Commons.

When abroad, Hertford in the diplomatic service and Conway in the army welcomed news from home and from a source they could trust. Hertford was especially concerned about possible changes in government that might affect his future. Conway, on the other hand, was so eager for letters that he once wrote, "Say anything, I don't care what" (vol. 37, p. 154). When in Paris or Dublin, Lord and Lady Hertford filled their letters with news of local politics and descriptions of their social life. From Paris, Hertford wrote repeatedly of the growing spirit of liberty.

General Conway's letters from the Continent differed from most of Walpole's correspondence, for they dealt with details of camp life and experiences at the front. Later, when Walpole was visiting in Paris, he kept Conway, then secretary of state, informed of significant developments in French affairs.

As for politics at home, Lord Hertford was most

cautious in his comments. Yet, late in his career, he wrote sadly, "We have contrived to make England less an earthly paradise than we were taught to esteem" (vol. 39, p. 422). Conway seldom disguised his liberal views. For instance, at a most delicate point in his career he opposed the ministerial stance on general warrants. Walpole was equally rash but had less to lose. By 1768 he had become so disillusioned with politics that he retired from Parliament, but he did not abandon his concern, and consistently denounced ministerial conduct that resulted in the loss of the American colonies.

In one way or another all the cousins were under obligation to Walpole in matters of time, money, or both. The letters of Henrietta, the younger sister, were devoted largely to seeking favors from her cousin whom she evidently found more approachable than her brothers. At Oxford, Lord Beauchamp, Hertford's eldest son, obtained Walpole's help in preparing a poem to submit to his tutors. As lord chamberlain Hertford sought Walpole's advice on the licensing of plays. But no one had as much reason for gratitude as Conway. Walpole once wrote to him, "You have ever been the dearest person to me in the world" (vol. 38, p. 381). And he proved his devotion on more than one occasion. When Conway's independence caused him the temporary loss of a regiment and also his place at court, Walpole altered his will in his cousin's favor. One might think that Walpole received little in return for his generosity; but it is safe to say that in spite of some disappointments the affection of these friends and their regular correspondence gave him untold satisfaction.

In addition to the specialized topics of diplomacy, war, and politics, these letters reflect the great variety of interests that occupied the time and thought of their authors. Life for Walpole, he confessed, was dull without "a new book, play, intrigue, marriage, elopement, or quarrel" (vol. 38, p. 511). He noted the changing seasons that lured him to Strawberry Hill in lilac time or back to London with the chills of autumn. And he duly recorded the weather. For him summer rain meant that his hay would be ruined but that his lawns would be green. Yet he seemed more interested in the decorative effect of the haystacks than in their practical use. As years passed he grew more philosophical about life—its sorrows and its ills—accepting with wry humor his frequent companion, the gout, thankful for the comfort of his "bootikins," and ever more grateful for his friends. They responded in kind.

Throughout these volumes Walpole's style seems more warm and natural than in some of his other correspondence. That is not to say that these letters lack the puns, witticisms, intriguing allu-

sions, gentle irony, and the humorous stories that made him a deservedly popular and quotable guest or host as well as correspondent. Those who replied appear to have affected the same style, with somewhat less success.

These volumes give new evidence of the unrivaled skill of the editors as, for example, in locating seemingly lost letters in such an unlikely place as Ceylon. More than half of these 687 letters are now printed for the first time. As in other cases, the introduction and preliminary explanations contribute to an appreciation of the letters themselves. An identification of others besides the principal correspondents would have been welcome, however; and the passage about the duke of Richmond raises the question as to why his letters were not included in these volumes if he was really considered "in the Conway connection" (vol. 37, p. xix).

All those who use this edition must necessarily admire the encyclopedic character of the footnotes. Any minor criticisms that may occur in the course of reading these volumes, such as the seemingly unnecessary repetitions or explanations of the obvious, by their comparative insignificance merely serve to emphasize the exceptional quality of the editing.

DORA MAE CLARK
Wilson College

F. O. SHYLLON. *Black Slaves in Britain*. New York: Oxford University Press, for the Institute of Race Relations, London. 1974. Pp. xi, 252. \$14.50.

This somewhat odd book, published for the Institute of Race Relations in London, is the fifth recent volume to appear on black history in Britain. Set beside the two volumes by James Walvin and the work of Howard Temperley, it fares badly as scholarship, and even when compared with Edward Scobie's recent *Black Britannia: A History of Blacks in Britain* (1972), which contains modest dollops of genuinely new material, it does little better. For *Black Slaves in Britain* is not an examination of the social role, or the economic functions, or of even public attitudes toward black slaves in Britain, despite its title. Rather, it is a close, often angry, examination of the way in which British common law approached the issue of slavery, and the volume focuses strongly upon the Somerset case of 1772.

The problem is that most historians of black, British, or imperial history know by now that Chief Justice Lord Mansfield's discharge of one black slave, James Somerset, did not end slavery or extend moral condemnation of it over the entire fabric of British law. How should anyone who understands the common law have expected that it did so? While much liberal cant was generated by

a number of careless historians who read too much into the Somerset case, by and large only journalistic histories accepted at face value the words attributed to Mansfield. "The air of England has long been too pure for a slave and every man is free who breathes it." Indeed, these words do not appear even in Capel Lofft's reports on the Court of King's Bench, and the quotation is, in fact, from John Lord Campbell, *The Lives of the Chief Justices of England* (London, 1857). All this, Jerome Nadelhaft, among others, demonstrated in 1966 in the *Journal of Negro History*.

Dr. Shyllon now demonstrates it again, with a fuller list of quotations from writers led astray by Mansfield, Campbell, or William Cowper's poetry. He demonstrates not once but three times that Sir Reginald Coupland was sentimental and wrong in his *The British Anti-Slavery Movement* (1933), and that it in turn gave rise to more making of myths. This argument, with numerous interesting and often caustic asides, is amply supported in the text, and again in an excellent note on sources. Yet the anger reveals a certain naïveté about how historians are led into error, and time and again an author's work is condemned as a "farce," as deliberately misleading, and unfortunate, in tones that speak of Roosevelt's man with the hoe. When one has finished this book, one feels that it could have made an excellent article that would have nailed the myths of Lord Mansfield to the door for all time; instead, by resorting to verbal overkill, it almost leaves one sympathetic to the poor, deluded historians who were so foolish as to believe what they read.

ROBIN W. WINKS
Yale University

JOHN NELSON TARN, *Five per Cent Philanthropy: An Account of Housing in Urban Areas between 1840 and 1914*. New York: Cambridge University Press, 1973. Pp. xiv, 211. \$23.50.

ANTHONY SUTCLIFFE, editor, *Multi-Storey Living: The British Working-Class Experience*. New York: Barnes and Noble, 1974. Pp. x, 249. \$17.00.

ENID GAULDIE, *Cruel Habitations: A History of Working-Class Housing, 1780-1918*. New York: Barnes and Noble, 1974. Pp. 363. \$15.75.

The history of working-class housing in Great Britain during the past century and a half has received much attention from historians in recent years. This extraordinary scholarly activity reflects both the quickened pace of study in urban history as well as the increased concern being given to the lives of the working class and the poor. The books under review make substantial contributions to both areas of research.

It was the nineteenth century that discovered the slum. The hideous conditions in which large segments of the working-class population lived became so manifest that reformers were compelled to do something to alleviate the situation. Humanitarianism certainly played a role in bringing about remedial action. But stimulus also came from the fear of what the *classes dangereuses* might do to upset the security of the possessing classes. At first the danger from the working class arose from the fear that their districts were the source of epidemic disease and death; thus, as all three books agree, the movement for improved housing developed as an adjunct to the public health campaign. Toward the end of the century, with the extension of the franchise and the coming of hard times, the alleged danger from what Gaudie calls "this unsafe people" was increasingly political, and it was self-defense that moved those in authority to seek new ways of providing better housing.

All three works generally agree that the solutions tried in the nineteenth century were inadequate largely because philanthropists, reformers, and politicians alike were prisoners of Victorian values. Two sacred principles stand out as determinants of housing policy and design: that the provision of housing was a matter of private enterprise and ought to bring a return on the funds invested, and that to subsidize housing for the poor—*ipso facto* the undeserving—out of taxes and rates was unthinkable. Given the economics of building, the improved housing, whether erected by building societies, speculative builders, or philanthropic model-housing associations, could only be made available to the better-paid workers. Even then, the barracklike blocks of tenements and the by-law streets that replaced the back-to-backs produced a grim environment for the working class.

Government intervened to regulate, but not to build. Legislation was permissive, however, and local authorities lacked the will to use their powers. The acts, most notably the Torrens and the Cross acts, were defective, and their implementation frequently aggravated the housing problem. Ultimately it was the intractability of the problem that led reluctant local authorities in the twentieth century to the necessity of building and owning homes. Yet, the solutions of this century, the garden cities and suburbs, and the high-rise blocks, as Tarn and Sutcliffe and his contributors point out, were only partial ones and indeed have spawned their own problems.

Although the three books, then, cover similar ground, they are quite different in emphasis and approach. Professor Tarn has concentrated on the work of the quasi-philanthropic model-housing associations, such as the Metropolitan Association

for Improving the Dwellings of the Industrious Classes and the Peabody Trust, and traces their impact on the planning of model communities such as Port Sunlight and Letchworth. Tarn's approach is primarily that of an architect and planner. He is at his best in analyzing the developments of building and community design and relating these to the larger history of the problems of working-class housing. Dr. Sutcliffe's book is a collection of essays by seven contributors, each dealing with an aspect of flat, or apartment, dwelling. These essays argue that flats are "un-English," and that until the present they were undesirable aberrations from the norms of English housing. Nevertheless multistory living grew in the nineteenth century as a relatively cheap way to provide better working-class housing, and in the twentieth as a way of adapting cheap housing to the suburban ideal. But in the postwar era the suburban ideal had degenerated into urban sprawl, and the high-rise blocks were called into existence to combat that problem.

Where the two previous books concentrate on special aspects of housing, Gaudie's study is a broad and more general survey. She deals with urban as well as rural working-class housing, with affluence and poverty, and private enterprise as well as government activity. She has not only incorporated the latest research in the field but has enriched her study with valuable sociological insights. While she condemns the Victorians for their callousness toward the poor and for their obsession with the sanctity of property, she nevertheless gives careful attention to the economic and political realities that made reform so difficult.

Inevitably there are areas of disagreement among the authors that raise important questions. Were housing problems inherited from rural conditions, or from the new industrial-urban conditions? Did the model-housing movement encourage or delay solutions to the housing question? Is flat-dwelling intrinsically a defective type of housing? What are the merits or demerits of concentration or dispersal of housing? Then, too, the books invite further study, particularly about the impact of housing policy and design on the lives of the working classes. How did the various patterns of housing affect family and social interactions? The authors offer not only new information, but pose provocative questions, and in so doing they have made important additions to the history of working-class housing.

ROBERT M. GUTCHEN
University of Rhode Island

PIERS MACKESY. *Statesmen at War: The Strategy of Overthrow, 1798-1799*. [London:] Longman. 1974. Pp. x, 340. £7.50.

In his third study of British policy and strategy Professor Piers Mackesy traces England's efforts to organize and direct the Second Coalition. Designed primarily by the foreign secretary, Lord Grenville, the coalition's objectives were indeed grandiose. The Alliance sought to overthrow the Republic, reduce France to her pre-1792 frontiers, and restore the monarchy.

With precision and clarity Mackesy describes Grenville's efforts to create the coalition. After months of tortuous negotiations, England failed to detach Prussia from her policy of neutrality but succeeded in concluding an alliance with Russia and a less formal working agreement with Austria. Grenville also devised and convinced his partners to accept a comprehensive military strategy. The campaign of 1799 called for coordinated campaigns in Italy, Switzerland, and southern Germany plus an amphibious assault upon Holland aided by local counterrevolutionaries. In 1800 Grenville planned two major offensives—one from Switzerland and the other from Holland. As the allied armies moved on Paris, royalist risings would seal the Republic's doom.

Given the conflict of ambitions among the coalition's members, it is not surprising that the Allies failed. Professor Mackesy describes the rivalries within the allied ranks and shows how the clash of political objectives adversely influenced military operations. Austria's decision to invade Belgium via the lower Rhine in order to gain bargaining strength at the war's end fatally weakened the coalition's strategic position. The Austrian move left a Russian army isolated in Switzerland and exposed it to a French riposte. Insufficient manpower, lapses of coordination between British and Russian commanders, and an overestimation of pro-Allied sentiment in Holland led to the failure of the Texel expedition.

Since the Continental powers did not consider France a threat of sufficient magnitude to cause them to put aside their ambitions in the interests of a common cause, Grenville had to settle for half measures and could not relate political considerations to military reality. Only years of continued French expansion would create the conditions for an effective coalition.

Professor Mackesy weaves together the strands of British policy strategy and operations. He avoids the danger of oversimplification without leaving the reader mired in a welter of trivia. He has successfully focused attention on an important though hitherto largely ignored period. The War of the Second Coalition set the scene for Napoleon's conquests and British resistance to the threat of French hegemony. Failure in 1799 forced England's leaders to undertake a major reappraisal of their arms and resources. I hope Profes-

sor Mackesy will continue his excellent studies of British strategy and policy into the period of the Napoleonic wars.

STEVEN ROSS
Naval War College

CHRISTOPHER HOWARD. *Britain and the Casus Belli, 1822-1902: A Study of Britain's International Position from Canning to Salisbury*. London: University of London, the Athlone Press; distrib. by Humanities Press, Atlantic Highlands, N.J. 1974. Pp. xiv, 204. \$12.00.

As in his previous book, *Splendid Isolation* (1967), Christopher Howard examines the realities that lie behind the conventional phrases historians have employed in explaining British foreign policy during the nineteenth century. In this slim and impressive volume, Howard finds little evidence for the use of general terms like isolation, a free hand, or the avoidance of binding alliances as principles of action. British foreign secretaries throughout the century, as most other European diplomats, said one thing and often did the opposite when it suited their purposes.

Through an examination of such episodes as the Belgium Treaty of 1839, the Tripartite Treaty of 1855, and the Luxembourg Treaty of 1867, it is demonstrated that British governments, regardless of party, gave formal and explicit guarantees when deemed necessary. The pragmatism that characterized British policy accepted or explained away the obligations as circumstances of the moment dictated. Even Lord Salisbury, the model of an isolationist foreign secretary, did not hesitate to incur responsibilities when he found it desirable. When he thought it inconvenient, he fell back on the constitutional argument that no British government could ever guarantee a specific pledge would be honored because of the inability to forecast the attitude of the House of Commons.

The true explanation for the absence of commitment to Continental powers, Howard argues, was simply the lack of a formidable army, and this made Britain less attractive to prospective partners. In addition no compelling reasons for an alliance appeared until the end of the Salisbury era. Despite his efforts to minimize the relevance of the customary generalizations, Howard admits Britain still enjoyed far more liberty of action than any of the major Continental powers.

In some ways, then, the author has attacked a straw man, for many historians have recognized that British diplomats based foreign policy on self-interest and used rhetoric for their own purposes. Thus the book should interest diplomatic historians, but some will doubtless regard it as an elaboration of the obvious.

RICHARD A. COSGROVE
University of Arizona

EDITH F. HURWITZ. *Politics and the Public Conscience: Slave Emancipation and the Abolitionist Movement in Britain*. (Historical Problems: Studies and Documents, 23.) New York: Barnes and Noble. 1973. Pp. 179. \$10.50.

The last decade has witnessed a revival of interest in British slave emancipation, inspired no doubt by the growth of black studies, but equally motivated by a sense that previous interpretations were defective. Eric Williams's *Capitalism and Slavery* put paid to the simplistic humanitarian determinism of Reginald Coupland, Frank J. Klingberg, and W. L. Mathieson, but few historians today accept Williams's equally simplistic economic determinism. Although Dr. Hurwitz is aware of the inadequacies of both approaches, she has avoided them at the cost of considerable confusion and contradiction. This emerges most distressingly in her chapter "Ideological Trends in Anti-Slavery Thought." While it is true that British abolitionism derived much of its moral passion from the late eighteenth-century evangelical notion of an abounding divine grace available to all men, it hardly follows that "Anti-Slavery thought cannot be divorced from the general body of Protestant theology" (p. 44). What about Luther quoting St. Paul on slavery? And what, for that matter, about the considerable body of Catholic literature critical of slavery, the influence of the Enlightenment, and of natural law with its roots in pre-Christian Stoic thought? The point that must continue to preoccupy the historian is that while most of the components of antislavery thought had been floating about Europe for centuries they did not form a coherent ideological attitude until the latter part of the eighteenth century. Why then, and why so powerfully in Britain?

Dr. Hurwitz also comes a cropper over another awkward aspect of the antislavery movement: the sharp contrast between the fiery concern of so many abolitionists for the condition of Negro slaves and their equally callous indifference to the state of the emerging English working class. She attempts to account for this by drawing a distinction between the emancipationists' "moralistic" narrowness and the broader "humanitarianism" of those concerned with "the great social questions relating to the industrial revolution" (p. 43). The possibility is not considered that what made many abolitionists "nonhumanitarian" was not so much their narrow orthodoxy or their preoccupation with morality as their political economy.

A subsequent chapter, "Anti-Slavery and the Emancipation Act," presents a summary, utilizing recent scholarship, of the pressure politics developed around the abolition, apprenticeship, and emancipation issues between 1831 and 1840. This is

more useful work. But Dr. Hurwitz seems willfully blind to the thrust of her own evidence. "It was not the industrial north aligned against the agricultural south, or the newly enfranchised boroughs versus the older counties and pocket boroughs that determined the nature of Anti-Slavery opinion. . . . The high principles, the immutable truths of the religious appeal penetrated into boroughs and counties of all sizes and in both the north and the south" (p. 55). This is the conventional wisdom that Harold Temperley's recent *British Anti-Slavery* has tended to reinforce. But a few pages later Hurwitz tells us that an "analysis of the vote of 24 July [on limiting apprenticeship] reveals where the greatest strength of the movement lay. There was formidable Anti-Slavery strength in the new boroughs. . . . Middle class in their orientation, . . . [the] 50 per cent increase in the electorate that resulted from the Reform Bill was solidly in the abolitionist camp" (p. 68).

The seventy-four pages of documents that follow the introduction include characteristic remarks from Wilberforce, Buxton, and Knibb on behalf of the slaves, the duke of Wellington and others on behalf of the planters, as well as a large extract from the Abolition of Slavery Act of 1833.

ALAN H. ADAMSON
Sir George Williams University

JANET ROEBUCK. *The Making of Modern English Society from 1850*. (Development of English Society.) New York: Charles Scribner's Sons. 1973. Pp. xii, 205. \$8.95.

Dorothy Marshall, editor of the series in which this volume appears, hopes that it will provide answers to the question "How did we get from there to here?" "There," according to Professor Roebuck, was a mid-nineteenth-century England as much rural as urban and divided along class lines. "Here," she maintains, is an urban England in which class distinctions have all but vanished.

Limited by the number of pages assigned her, the author can only hint at the reasons for the change. Although, for example, she frequently refers to increasing state intervention as an important factor in the improvement of working-class living standards, she nowhere discusses changing attitudes toward this phenomenon, as social-service state gave way to welfare state, as the principles of the Charity Organization Society fell before those of the Minority Report of the Poor Law Commission. Nor do her occasional summaries of the economic factors determining the course of industrialization and urbanization mention the consequences of the rise and fall of Britain's second empire.

Able to devote relatively little space to an analysis of the way in which the England of 1850 became the England of 1970, the author prefers to reiterate her contention that the events of the intervening century have effaced all but the last vestiges of class. She does little to suggest how the Victorian working class was "made"—Chartism, although essentially a pre-1850 movement, might have rated at least a mention. The "unmaking" she attributes to the growth of a white-collar class, the spread of mass culture, and constantly expanding educational opportunities. In building her case she ignores evidence to the contrary: the work of Brian Simon and of David Lockwood and J. H. Goldthorpe, for example, which argues that class distinctions remain very much a part of English life and contribute to its complexity and vigor in a way the author fails to appreciate.

One is left wondering who is expected to read this book. Dorothy Marshall claims it is not meant as a text, but rather as a "background book." But background is what one constantly craves as one reads it; an occasional halt in the narrative for an analysis of this or that historical phenomenon; a fuller and more imaginative use of primary sources—sadly skimmed; the inclusion of photographs—there are none. Smoothly and often thoughtfully written, the book unfortunately appears to answer no one's particular needs.

STANDISH MEACHAM
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Austin

B. A. ROZHKOVA. *Angliiskoe rabochee dvizhenie, 1859-1864 gg.* [The English Workers' Movement, 1859-1864]. (Akademiia Nauk SSSR, Institut Vseobshchei Istorii.) Moscow: Izdatel'stvo "Nauka." 1973. Pp. 234.

Was it a time of lost direction and spent energy—or a vitally constructive period solidly preparing the next advance? Were these years fruitful thanks to—or in spite of—the Chartist legacy? Which counted more, the cautious "New Model" unionism, the quickened zeal for political action revived by men like Robert Applegarth, or simply the desperate strike activism of the rank and file? Opposite answers have long abounded in the scholarly literature.

According to Rozhkova the period registered advances—despite determined aggression by both employers and a government linked to reaction abroad, and despite the defective leadership of labor. Those leaders, seeking to defend the privileges of the labor aristocracy, forsook greater tasks—only to find their lesser-defensive tasks hopeless; it took the course of events, and pressure from below,

to teach them the need for united political action. Rozhkov tests labor leaders many ways: internationalism, militancy, suspicion of the bourgeoisie, and above all, awareness of the largest question of all—how to advance the day when a unified, class-conscious working class could forge instruments for class politics of its own. The case is not stated for the alternatives. However, the alternatives of joint action with middle-class spokesmen for gradual electoral reform and limited unionizing campaigns with minimum recourse to strikes are mentioned.

Turning to evaluation, all depends—for me—on whether comparisons are with the “east” or with the “west.” Measured against works from the Webbs through Frances Gillespie and the Coles to today’s Royden Harrison, this is two-dimensional stuff, a stock morality play acted out against thin hangings; the rich tapestry of actual working-class—and interclass—debate, and the personal complexity of major figures like Applegarth, George Potter, and John Bright, are hardly suggested; the huge detail on union affairs, spread at large in the Webbs’ pages, seldom appears. Yet, for volume of detail within its chosen material, and for relative freedom from ritual quotes of Lenin and Marx, it comes off better than some products of the Akademii Nauk. The heavy reliance on one newspaper, Potter’s *Bee-Hive*, reflects Rozhkov’s framework and conclusions, but the explicit acknowledgment of the role of Radical liberals in suffrage reform and the conspicuously minor role accorded to Marx in founding the First International are distinctive.

PAUL B. JOHNSON
Roosevelt University

GERTRUDE HIMMELFARB. *On Liberty and Liberalism: The Case of John Stuart Mill*. New York: Alfred A. Knopf, 1974. Pp. xxiii, 345, v. \$8.95.

The reviewer who thinks, as I do, that *On Liberty and Liberalism* is a wrong-headed and mean-spirited piece of work is under a particularly stringent obligation to insist on its virtues. It is an easy and a pleasant obligation to fulfill, for the virtues of the book are numerous and obvious. Miss Himmelfarb is well known as a very readable historian of ideas; she conveys a sense of intellectual and moral urgency, but does so with enviable lucidity and calm. She possesses a considerable talent for thumbnail intellectual sketches, and here she employs that talent to bring in many of Mill’s nineteenth-century critics without crowding the stage or confusing the issues. And it goes without saying that this book is a highly intelligent one; once the oddity of *On Liberty* has been noted and elaborated, Himmelfarb provides what is in principle the right sort of explanation of it.

A crude summary of what Himmelfarb believes is that *On Liberty* is the textbook of radical individualism; its leading principle is the individual’s absolute sovereignty over himself, and the corollary is society’s absolute impotence; this moral stance conflicts with the views of the “other” Mill, who insisted on the futility of laying down any single, simple principle to determine the limits of social and political action and offered to society a much larger role in the training and disciplining of its members. *On Liberty* seems to envisage the individual as permanently at odds with society, and permanently threatened by the dead weight of public opinion. Since the 1850s were not years of peculiar repressiveness in English social life, *On Liberty*’s fear of moral oppression needs explaining. The explanation is to be found in the relationship between Mill and Harriet Taylor; the extreme individualism of the essay stems from her concentration on the social disabilities of the would-be emancipated female; its simple-minded espousal of one absolute principle stems from her attachment to absolutes. Himmelfarb ends with a coda indicting the twentieth-century individualism that she believes has the paradoxical effect of encouraging too much state intervention in some areas and too little in others; the best safeguard of liberty is not a belief in its absolute value, but an appreciation of a plurality of values of the kind one finds in Burke or in the “other” Mill.

Himmelfarb’s case will be familiar to those who have read her other essays on Mill; indeed much of what appears here is no more than a repetition of what has appeared already, and within the book itself there is a good deal of repetition. It is more damaging that Himmelfarb never questions her own belief that *On Liberty* is as much at odds with the corpus of Mill’s work as she says. But since she first put forward her account of the “two Mills,” a number of writers have defended the view that there is only one Mill, and that *On Liberty* is consistent with Mill’s other more obviously Utilitarian writings. Himmelfarb quite properly concentrates on nineteenth-century commentators and therefore ignores all this; but even Mill himself went to some lengths to deny Himmelfarb’s view of what he was doing. He insisted that the right to liberty was not a natural right to individual sovereignty, but was derived from utility. Mill’s view may be hard to swallow; but it is not a view that sets us the task of explaining why *On Liberty* is so unlike most of Mill’s other essays.

On Liberty has a very obvious place in Mill’s career as a Utilitarian. Utilitarianism is an ethics that relies very heavily on the sanctions of public opinion; Mill feared that this reliance threatened the Utilitarian goal itself. It is the Mill of the essay on Bentham who asks how we can take an interest

in each other's virtue without mutual oppression; *On Liberty* suggests some of the answers. All this is neglected by Himmelfarb, who, as a result, caricatures Mill's somewhat complex position and turns it into just the doctrine of mutual indifference that Mill said he deplored. Himmelfarb is thus inaccurate in her account of what *On Liberty* contains and unfair to Mill in offering so incomplete a picture of his views about noncoercive social relationships. Since she starts off by drastically misconceiving the nature of the essay she is trying to explain, it must, I fear, follow that much of the effort and ingenuity that *On Liberty and Liberalism* so abundantly displays has been a waste of time.

ALAN RYAN
New College,
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D. A. LOW. *Lion Rampant: Essays in the Study of British Imperialism*. (Studies in Commonwealth Politics and History, number 1.) London: Frank Cass; distrib. by International Scholarly Book Services, Portland, Ore. 1973. Pp. x, 230. \$12.50.

C. C. ELDRIDGE. *England's Mission: The Imperial Idea in the Age of Gladstone and Disraeli 1868-1880*. Chapel Hill: University of North Carolina Press. 1973. Pp. 288. \$10.95.

IAN M. DRUMMOND. *British Economic Policy and the Empire, 1919-1939*. (Historical Problems: Studies and Documents, 17.) New York: Barnes and Noble. 1972. Pp. 241. \$13.50.

For sixty years theories of and studies in the theory of imperialism have made the shelves sag in a dozen languages. Today historians of all nations are readier to take a closer look at the empires themselves without wincing. They are prepared to find out what went on in these properties before issuing the automatic verdict, acceptable on platforms from China to Peru, that imperialism was a wrong inflicted on humanity and that nationalism was a right enforced by a fraternity which would not conform to that wrong or agree to its continuance. Finding out what went on is, however, not easy to do, since most of it was not documented. The older-fashioned historians of empire do not help much. They told the stories of expansion, the deeds that won the empire, with anecdotes now and then about the bewilderment of natives who wondered what it was the white men wrote all day at their desks. The best of them allowed that they were dealing with both a phenomenon and a puzzle. Why was the alien control, so physically flimsy, so readily accepted? Why were all colonial governments multiracial? They knew that in India the millions, if minded to do so, could easily have swept away the handful of foreigners who ordered

their affairs. Why were they not minded to do so? Gandhi, on his return to India from South Africa in 1916, saw the point almost at once: Indians had only to say no. Imperial officials knew of both the question and the answer and were careful to refer to neither in their memoirs. But they knew, too, that the reason for their own success was not what Hilaire Belloc thought it was: "We have the Maxim gun, and they have not."

The gun could help to establish law and order, but it could not enforce it. In imperial terms law and order meant peace and order: no abstract matters of justice were involved. Empire was a system, a way of making the affairs of an expanded world work. Progress was the vision, but organization was its base. Order was the basis of all social action. If there were few soldiers and policemen around, that itself proved the point: the mass of the people agreed with the principle, not because it was alien or because they loved foreigners but because it was so obviously useful, as any Chinese merchant in Malaya could have told anyone who asked. The common sense of this—clear to the imperial memorialist, his readers, and his juniors out in the midday sun—was later overlaid by the argumentation of those who declared that since power corrupted everyone all the time (not what Lord Acton said), anything established by means of its use could do nobody any good at all, ever.

Nowadays the notion that a world order of some sort is essential to keep chaos at a distance has won over even those who believe that establishing an empire is a poor or fascist way of going about it. Systematization, modernization, and development are names for the exercise of power which have not attracted the opprobrium that has been attached to imperialism, because the vision they follow is acceptable everywhere. Three new books consider this question of intent. D. A. Low calls his book on the British Empire *Lion Rampant*—but "Lion Couchant" would make a better title, since Low is discussing the ways by which empire established its control over the habits, and therefore the minds, of its subjects. He deals with acceptance and conformity, both of which lie in areas of darkness. The strangest imperial innovation introduced was that of a social framework larger and more powerful than that which every people everywhere had already made to suit themselves and their own situations. Efficiency was its magic, and the assurance of those who organized it made them the wizards of their day. Low examines the ideas of authority, development, and Christianity—seen from below as a better guide to success here and now and there and later. He insists there are other, more rewarding themes for scholars of imperialism than the impetus of the West. The ground to be investigated is local ground, not what the controllers

thought but what the controlled wanted. Societies among which imperialists descended had their own reasons for reacting as they did. Why Afghans behaved one way and Matabele another will not be discovered from studying any theory of imperialism.

Low looks at unmapped territory and skillfully draws a chart. C. C. Eldridge's book treads well-known ground, but he takes up a stance not far from Low. Instead of weighing theories or the persons of Ronald Robinson, John Gallagher, D. C. Platt, and Oliver Macdonagh in the scales, Low advises a look at mid-Victorians' intentions. These were men unconvinced that empire was either a workable or even a useful system, but they were ready to be shown otherwise. By the end of the century the people who showed them had won out because they had lived longer, to see the days when open doors and free trade no longer commanded beneficial results and to deal with competitors who thought more of force than of example. But if mid-Victorian imperialism was, as Eldridge allows, "a very weak affair," that was because it had no reason to be strong. This well-researched book deals fairly with a generation fortunate in having so many options and intelligent in the way it handled them.

To read Ian M. Drummond's book on imperial ideas during the interwar years is to find these same themes underlined. Here were men who misunderstood their few options. They dreamed economic dreams of self-sufficiency that had no basis in political circumstance. They emblazoned "The Empire" on a flag under which they intended to exploit an elusive "trade"; but these days had gone. Drummond tells why, clearly and sometimes kindly. Empire still has its ground for tilling, as these three excellent historians prove.

A. P. THORNTON
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RICHARD D. WOLFF. *The Economics of Colonialism: Britain and Kenya, 1870-1930*. (Yale Series in Economic History.) New Haven: Yale University Press. 1974. Pp. xv, 203. \$9.75.

The antislave campaign in East Africa launched Britain into a period of colonization and a total reorganization of the region's economy. The efforts of Consul John Kirk during the 1870s to abolish the slave trade culminated in British acceptance of Livingstone's earlier idea that commerce and Christianity were the joint solutions. In 1887-88 the Imperial British East Africa Company was formed, and formal European penetration and administration of East Africa began.

Many economists and historians have claimed

that Africa's role in Britain's economic empire was insignificant. Wolff provides figures and tables to prove that the economic benefits derived by Britain could not have been gained if she had not maintained direct control over her colonies. African colonies became leading suppliers of metals, foodstuffs, and other raw materials, so that by 1913 over 80 per cent of the value of British imports originated in Africa. It is interesting to note, however, that after devoting over 20 per cent of this volume to such statistics, the author leaves the reader wondering what the role of Kenya was. Since two of his conclusions are that European farming was inefficient in Kenya and indigenous farming collapsed under the pressure of "forced labor" on European farms, and since Kenya is without the natural deposits of copper and tin that benefited other British colonies, how does this case study of Kenya disprove earlier theories?

During the administration of Commissioner Charles Eliot, European immigration into the Kenya "white highlands" gained impetus. A chapter is devoted to the colonization of land in Kenya, and another chapter details the determination of crops. Groundnuts and ostrich feathers were suggested by settlers in 1903-05, and a cotton experiment failed. Major acreages were, however, devoted to successful planting of maize, wheat, coffee, and sisal.

In order to provide labor for European farms, the British administration taxed Africans so heavily that their only means of earning sufficient cash was to work for a European planter. This was followed by initiation of a pass system that required employment on a European estate. Wolff further indicates that Africans were forced to work for Europeans when the British government refused to aid them with modern technology, loans, or credit. While this may be true for the region of the Kikuyu, it does not apply in Kavirondo where the annual and quarterly reports from 1906-25 indicate that the government introduced various seeds and technical assistance to raise the value of crop production. From 1908 to 1912, for example, the value of exported agricultural products raised in Kavirondo rose from 60 tons to over 28,000 tons. This increased production, however, did not give the African a higher standard of living; it merely enabled him to pay his taxes.

Although the Asian was an important element in the economic growth of Kenya, his role is intentionally omitted. What Wolff has done, therefore, is to present a study of British colonialism and the effects of that colonialism on the British home government, British settlers, and the British economy. This is an interpretation that appears fresh. While not condoning colonization by the British, the case is made that at the time it took

place, colonization of East Africa was necessary for the British to survive economically and compete in world markets.

HARVEY G. SOFF
Decatur, Georgia

MICHAEL ROE. *Kenealy and the Tichborne Cause: A Study in Mid-Victorian Populism*. [Carlton:] Melbourne University Press; distrib. by International Scholarly Book Services, Portland, Ore. 1974. Pp. xiii, 255. \$14.85.

Michael Roe's subject is not the Tichborne case, the legal *cause célèbre* of the later 1860s and 1870s that raised the question whether the claimant was truly Sir Roger Tichborne, heir to an ancient English baronetcy, or rather Arthur Orton, son of a Wapping butcher. Douglas Woodruff's *The Tichborne Claimant* (London, 1957) provides the most complete account of that "Victorian Mystery" while suggesting that a good deal of mystery remains. Roe's subject is rather "the cause," the movement of popular support led by Edward Kenealy, the Irish-born London barrister who was by avocation a poet, a novelist, and an eccentric theologian who privately suspected that he was a "Messiah," twelfth in a chain that—via Genghis Khan, Mohammed, and Christ—stretched back to Adam.

His failure in 1874 to save the claimant from conviction for perjury and from jail did not prevent Kenealy from winning a surprise parliamentary bye-election victory the following year and from temporarily leading an inchoate popular movement of local "Magna Charta Associations." They favored freedom for the claimant, triennial Parliaments that excluded lawyers, land-law reform, votes for women ratepayers, and the monarchy on the one hand, while opposing both major political parties, Roman Catholics, capitalist stockjobbers, and compulsory vaccination, education, and liquor licensing on the other.

In a provocative and valuable seventh chapter, the author places the Tichborne agitation of the 1870s within the context of a neglected antielitist, antistatist, and often highly emotional English "populist" tradition—with roots in the seventeenth century and with exemplars ranging from the Queen Caroline supporters of 1820 and the Feargus O'Connor Chartists of the 1840s to the down-and-outers depicted by George Orwell in the 1930s. The rest of the book, though based on careful research, is unfortunately poorly organized and written in a style devoid of either clarity or grace.

WALTER L. ARNSTEIN
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GILLIAN SUTHERLAND. *Policy-Making in Elementary Education, 1870-1895*. (Oxford Historical Monographs.) New York: Oxford University Press. 1973. Pp. x, 391. \$21.00.

Recent work on the history of education in Britain marks a new, revisionist stage in that study; one may speak of a New School, in which Sutherland, the author of a manifesto in its behalf, holds a leading place, which is confirmed by the present book, a reworking of his dissertation. His first love, however, appears to be government and administration, education serving to test the "revolution in government" thesis. He favors Oliver MacDonagh over Henry Parris but finds that both ideology and the administrative process itself were important in policy formation.

In this masterful analysis of the interplay of institutions, personnel, and policy, a contrast quickly emerges and runs throughout. The politicians, whether lord presidents of the council or local school board members, originated all significant policy changes; the Education Department, Lingen's "passionless bureaucracy," exerted a passive, often deadening influence. This contrast is true enough but is overdrawn and smacks too much of our contemporary cant that bureaucracy is a bad thing.

Part 2, "Policy-Making" to 1888, unlike part 1, "Policy-Makers," deals with matters more familiar to historians of education. The establishment of compulsory attendance by the Acts of 1876 and 1880, and the creation of the habit of school attendance, are detailed; the issue of free or what Gladstone persisted in calling "Gratuitous Education" is considered but not carried to an appropriate point owing to Sutherland's questionable chronological division. One has the same sensation of being left up in the air by his analysis of the grant system and of the modification of payment by results. Here, too, the exposition breaks off at 1888 so that the final abandonment of payment by results and the abolition of fees are left over to part 3, "Policy and Policy-Makers" to 1895. Here A. H. D. Acland, surprisingly in a book quite devoid of heroes—indeed iconoclastic of the education reputations of, for example, W. E. Forster, A. J. Mundella, and Matthew Arnold—emerges as the uniquely whole-hearted reformer.

Why end at 1895? The Cockerton Judgment of 1899 and the subsequent reorganization under the legislation of 1902-03 form the logical terminus and would not lengthen the book unduly. This contention finds support in Sutherland's account of Treasury efforts to curb education costs—see his earlier essay, "Administrators in Education after 1870," in *Studies in the Growth of Nineteenth-Century Government* (1972)—so, too, that invention of the school boards and the higher grade schools, which

cannot be ignored, especially as they fit Sutherland's thesis on policy innovation so neatly.

Sutherland's suggestion that the school boards enjoy too "good a press" in the writings of Brian Simon and Eric Eaglesham commands assent. On Robert Lowe he is a little old-fashioned and does not reckon with Christopher Duke's "Re-Appraisal." One misses in the bibliography such stalwarts as John W. Adamson, C. Birchenough, G. A. N. Lowndes, and Frank Smith. Sutherland's mastery of a vast range of primary sources does not absolve him of this common failing of revisionists. The book's focus is almost solely on England. To that extent the challenge of the New School has not been met.

In many respects, then, this is a profound and important book, but in the end one parts from it unsatisfied; I hope it will serve Sutherland as a gateway to a work of larger scope.

FREDERICK M. SCHWEITZER
Manhattan College

G. N. UZOIGWE. *Britain and the Conquest of Africa: The Age of Salisbury*. Ann Arbor: University of Michigan Press. 1974. Pp. xii, 403. \$14.00.

This book pretends to do too much. Its jacket heralds a "radical reinterpretation of British imperial policy in Africa," and Professor Uzoigwe asserts in the preface that his study "breaks new ground in methodological and conceptual terms by discussing the intellectual and socio-politico-economic problems of the second half of the nineteenth century against the background of European diplomacy and African history." But while *Britain and the Conquest of Africa* raises several issues and proposes many themes that might have been pursued with profit, it develops few of these with any thoroughness or rigor, and the reader soon discovers that the introductory fanfare was unduly exuberant. Well researched and copiously documented, this substantial monograph will interest all researchers in the field of nineteenth-century European imperialism, but it does not invite comparison with a historiographical landmark such as Ronald Robinson's and John Gallagher's *Africa and the Victorians* (1961).

Uzoigwe's study focuses directly on the figure of Lord Salisbury, British prime minister and foreign secretary for a combined total of fourteen years during the last quarter of the nineteenth century; it is the author's main contention that "Salisbury's claim to historical fame lies not in foreign policy so-called but in his management of the conquest of Africa." Others may not choose to share that view, but Uzoigwe illustrates in fine detail the patient mastery with which his protagonist conducted

British policy in the course of the African partition, and his chapters on the drawn-out rivalry with France for control of the Niger, an episode that all too often gets short shrift, are especially instructive. Subsequent sections, "East and Central Africa" and "Egypt and the Sudan," most notably the former, seem to have been rendered with a less-sure sense of purpose and authority.

Appropriately, it is the climactic years of Salisbury's last term at the foreign office (1895-1900) that receive the greatest emphasis in Uzoigwe's book. The author seeks to demonstrate, however, that Salisbury, far from being a "reluctant imperialist" at any stage of his career, was out to gain the lion's share of Africa for England from the start. That argument rests precariously on one piece of objective evidence: an exposition on "the pacific invasion of England" written by Lord Salisbury in 1878 with exclusive reference to the Middle East and Turkey. Uzoigwe has been captivated by his documents and sometimes tries to make too much of them.

HERBERT PAUL MERITT
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ALFRED F. HAVIGHURST. *Radical Journalist: H. W. Massingham (1860-1924)*. (Conference on British Studies Biographical Series.) New York: Cambridge University Press. 1974. Pp. xv, 350. \$19.50.

This book is a notable addition to a series that has, with one other exception, so far proved undistinguished. The subject is well worth writing about. Massingham was an editor in a golden age of editors. A. G. Gardiner of the *Daily News*, J. L. Garvin of the *Observer*, C. P. Scott of the *Manchester Guardian*, J. A. Spender of the *Westminster Gazette*, St. Loe Strachey of the *Spectator*—these were his contemporaries. Moreover, as editor of the *London Nation* he was for more than a decade the arbiter among a group of exceptionally gifted liberal intellectuals. H. N. Brailsford, J. L. Hammond, L. T. Hobhouse, J. A. Hobson, C. F. G. Masterman, H. W. Nevinnson—these were his friends and colleagues. It is a curiosity of British historiography, in sharp contrast with American practice, that the work of journalists, publicists, and intellectuals should so largely have been ignored. There is a current fashion for writing about "high politics" in a way that effectively dismisses the influence of ideas. But the intellectuals, for whom politics was largely about ideas, cannot be assimilated to such a method. And while it is welcome that Professor Havighurst has now written fully about Massingham, it is somewhat disappointing that he does not give the formal content of his political thought more attention. Attempts

to reinterpret and reformulate liberalism as a general theory were matters of serious concern on the *Nation*, and it does not do justice to this side of Massingham to deprecate this as a superficial "verbalisation of Liberalism and Radicalism." This caveat aside, the study fulfils all reasonable expectations. Massingham did not leave a large collection of papers, and the author's research has been both painstaking and eclectic in filling as many gaps as possible. Many archives have been consulted; a comprehensive use of the files of the *Nation* has yielded much of value; stray references in a wide range of publications have been noted; and the level of accuracy is generally high. The result is a convincing reconstruction of the pattern of Massingham's activities. The balance of the story is held judiciously, and the easy availability of extensive source material at some points is not allowed to distort it. The author's claims for Massingham are neither excessive nor unduly modest, and the solid merits of this book ought to earn it a place in all bibliographies of the period.

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KENNETH D. BROWN, editor. *Essays in Anti-Labour History: Responses to the Rise of Labour in Britain*. [Hamden, Conn.:] Archon Books. 1974. Pp. viii, 409. \$15.00.

The rise of Labour along with the parallel, if not consequent, decline of the Liberals must surely be the central issue of British politics in the twentieth century. Yet we know almost nothing about the response to Labour, nor, until recently, was the question even raised. Labour seemed so woolly and so tame that it was difficult to conceive of anyone becoming frightened at the prospect of Labour taking office. Or if they did, it seemed reasonable to assume that politics continued much as before. The British, unlike the French or the Italians or most peoples of the world, are supposed to have acknowledged the legitimacy of political opposition: would they not respect all parliamentary opponents regardless of their ideology or program? Somehow we took this for granted. Though we argued about the inevitability of the rise of Labour and the relative importance of Liberal factionalism or tactical "mistakes" such as the Lib-Lab electoral agreement of 1903, we forgot that Labour's rise to power, whatever the reasons, had to be permitted by Labour's opponents. Their permission was granted, to be sure. But this was in spite of, not in the absence of, a great many efforts to keep Labour out.

Kenneth D. Brown's book deals with these efforts and the reasoning behind them. It suffers as a volume from a tendency to confuse socialism with

Labour, never explaining when Labour was first perceived as a threat, whether the threat was ideological or electoral in nature, or whether it was the same for both Liberals and Conservatives. It covers, on the other hand, a much broader period of time than Maurice Cowling, *The Impact of Labour, 1920-1924* (1971), the only other work on this subject. The essays—there are twelve of them—are good, and they let us draw certain conclusions, however limited they may be.

From Christopher Kauffman's "Lord Elcho, Trade Unionism and Democracy," J. W. Mason's "Thomas Mackay: The Anti-Socialist Philosophy of the Charity Organisation Society," D. J. Ford's "W. H. Mallock and Socialism in England, 1880-1918," and N. Seldon's "Laissez-Faire as Dogma: The Liberty and Property Defence League, 1882-1914" it would seem, for example, that increased labor militancy and the formation of a Labour party merely intensified pre-existing fears of state intervention. Landed aristocrats, Conservatives, old Whigs, model employers, publicans, place seekers, Spencerian individualists, elitists, and traditional do gooders all felt threatened by Gladstone's legislation. The question is whether their response was entirely negative. Edward Bristow, "Profit-Sharing, Socialism and Labour Unrest," and Kenneth D. Brown, "The Anti-Socialist Union, 1908-1949," deny that it was. But the qualifications they discuss are minor, as they themselves point out, at least where the Conservatives are concerned.

With regard to the Liberals, Michael Bentley in "The Liberal Response to Socialism" reminds us of certain political considerations, the danger of adopting a positive policy that might alienate too many supporters and the difficulty of adopting a negative, antisocialist position when Conservatives were far more convincing. According to Bentley, the Liberal response was ill conceived and inept: if Peter Cline, "Eric Geddes and the 'Experiment' with Businessmen in Government, 1915-1922," is correct, then the hardening opposition to Labour, inept or otherwise, cannot be blamed on the businessmen recruited by Lloyd George.

So much for the postwar years; the remaining essays deal with an earlier period. All agree that the Liberals were holding their own against Labour; they differ as to why. Kenneth O. Morgan, "The New Liberalism and the Challenge of Labour: The Welsh Experience, 1885-1929," suggests that it was the old liberalism that flourished and that it continued to flourish until specifically Welsh grievances were eliminated, ironically through the passage of Liberal reforms. In contrast, Edward David, "The New Liberalism of C. F. G. Masterman, 1873-1927," and Chris Wrigley, "Liberals and the Desire for Working-Class

Representatives in Battersea, 1886-1922," note the importance of Liberal reforms and the willingness of Liberals to meet Labour halfway. So does Roy Douglas, "Labour in Decline, 1910-1914." But in demonstrating how effectively the challenge of Labour had been contained, he calls into question one of the points in his *History of the Liberal Party* (1971), namely that the Lib-Lab electoral pact of 1903 was a mistake from the Liberal point of view. In the context of his present essay, the pact might better be seen as contributing to the success of the new liberalism—a wise concession that ought to have been made sooner than it was.

BARBARA MALAMENT
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JOHN GOOCH. *The Plans of War: The General Staff and British Military Strategy c. 1900-1916*. (A Halsted Press Book.) New York: John Wiley and Sons. 1974. Pp. xiv, 348. \$14.95.

A laudatory foreword by Michael Howard should ensure this book the attention it deserves. Both its limits and achievements require noting. The first half of the work traces in detail an essentially skimpy and incompletable process. The theme is the grafting on to the much-troubled central organization of the British military establishment an adaptation of the German general staff system—"The Brain of an Army." This section, organizational in emphasis, can profitably be read in conjunction with Brian Bond's *The Victorian Army and the Staff College, 1854-1914* (1972), which takes one away from Whitehall not only to the staff colleges of Camberley and Quetta, but also to the test of battle on the western front. While the maneuvers of the ubiquitous Lord Esher and others are carefully traced, as are the evolutions of staff rides and conferences, training manuals, and professional journals, the overall effect is one of tentativeness. The general staff never really seems to have taken hold. This is a valuable conclusion but in itself less than gripping.

The real contribution of the work for most readers is in the chapters on military planning from the Boer War and the Anglo-Japanese alliance to 1914. Deliberately de-emphasizing the traditional attention to what Howard has called the Continental commitment, Gooch focuses first on the "strategic reorientation" following on the Japanese alliance, then discusses the defense of India and Egypt. The important implication to be drawn from his analysis is that naval doctrine on the "command of the sea," combined with the existence of an alliance with Japan, freed the army from concentration on defense against invasion and enabled it to think

less in terms of raids and more in terms of large-scale operations. Japan, rather than France, "seemed to offer the surest pattern for the future course of military co-operation with other powers" (p. 191). After a brief review of the projected British role in Franco-German hostilities, the book concludes with the dissolution and reconstitution of the general staff in 1914-16.

Gooch writes well and with obvious intelligence, though his style is somewhat subdued. More important, one might wish that he would take the time to assess the bearing of political and diplomatic factors on his two subjects. This sober monograph, based on a variety of private and official primary sources, is to be welcomed as an indisputable contribution to the history of British military organization and planning before 1914.

PAUL GUINN
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Buffalo

J. M. WINTER. *Socialism and the Challenge of War: Ideas and Politics in Britain, 1912-18*. Boston: Routledge and Kegan Paul. 1974. Pp. ix, 310. \$17.00.

The virtues of this book are, unfortunately, outweighed by its defects. The bulk of the volume concerns the ideas of Sidney and Beatrice Webb, R. H. Tawney, and G. D. H. Cole first as they evolved from 1912 to 1914, in response to the labor unrest of the prewar years, and, in the second half of the volume, as they developed under the impact of war. Winter precedes the latter half of his study with an epigraph from Rosa Luxemburg concerning the delusion that socialist thinking could emerge unchanged by the war. The author, however, ends by concluding just this: the war had "neither a creative nor a decisive effect on the development of socialist theory in Britain." The curious inappropriateness of the epigraph only accentuates the reader's doubts concerning the meaning and viability of the study.

Winter insists that the real influence of the war was to be found on the "practical" front, for example, in the decision of socialists to use the Labour party as a vehicle for their ideas, and in the victory of the Webbs' view of socialism in the party's constitution of 1918. But his discussion of these questions constitutes the smallest and least persuasive part of the volume. Certainly, there is little concern with leading movements of British politics in the decade before the war that seem important to his thesis: we hear virtually nothing, for example, of the efforts of Chamberlain to turn the Tories from free trade, or of those of the New Liberals to bring their party onto a more "socialist" road. Nor, for that matter, does the author tell

us about the wartime movements toward state intervention, described so well by the late Professor S. J. Hurwitz.

All in all, the thinkers to whom he devotes himself seem to live in a strangely isolated world, largely that of the English labor and socialist movement in the six years from 1912 to 1918. Sometimes this myopia has a ludicrous aspect. For example, Sir Oswald Moseley's racism and anti-Semitism are attributed to the Labour party, a British socialist sin growing out of the racism of the Webbs. Surely Sidney Webb was not the only source of contagion to which a wealthy baronet might have been exposed after his desertion of the Conservative party for Labour.

Still, the author displays some good insights in his portrait of Tawney's Christian socialism, and Cole's gild socialism. Winter is least original on the Webbs, to whom he gives the bulk of his attention, and against whom he seems to be waging a vendetta.

BERNARD SEMMEL
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ARTHUR J. MARDER. *From the Dardanelles to Oran: Studies of the Royal Navy in War and Peace, 1915-1940*. New York: Oxford University Press. 1974. Pp. xiv, 301, 5 charts. \$21.00.

Though four of the five chapters in this somewhat expensive work revise previous publications, their collection is most welcome. Chapter 5, about a third of the work, is a new and masterly study of one of the largest actions of the Royal Navy—four capital ships on each side—during World War II. Marder sees "Oran, 3 July 1940: Mistaken Judgment, Tragic Misunderstanding, or Cruel Necessity?" as a cruel necessity. A question raised by chapter 3, "The Royal Navy and the Ethiopian Crisis of 1935-1936," is the influence of France's letdown of 1935 on British thinking in 1940. Chapter 4, "'Winston is Back': Churchill at the Admiralty, 1939-40," may end Marder's debate with the official historian, S. W. Roskill, over Churchill's "undue" interference in naval operations. Marder finds that this interference was primarily psychological and "in the main extremely beneficial," and that "it was the *impression* the First Lord gave, far more than specific acts of interference with naval operations."

Chapter 2, "The Influence of History on Sea Power: The Royal Navy and the Lessons of 1914-1918," finds the submarine danger, naval aviation, and amphibious operations neglected for replays of Jutland. Were the reasons, again, partly psychological? Chapter 1, "The Dardanelles Revisited: Further Thoughts on the Naval Prelude,"

fixes on one line of unswept mines and almost convinces a believer in the maxim that something always goes wrong with such inexperienced forces. Why an "if only" Dardanelles rather than another Jutland? The answer may well be Winston fascination.

Charts, index, and footnotes are admirable, though "the serious reader" who wants to "consult Captain Roskill's provocative, and in places, amusing, article" has to dig back from note 142 to notes 137 and 21 (ch. 5) to find it. Pictures are mostly of personages, an unconscious revelation, perhaps, of Marder's mastery of the personalities of this era. Because of this, he might go into some additional psychological issues. He has dealt with "the interwar obsession with a fleet action." Was there a similar obsession—in addition to the political reasons for it—with the Mediterranean? The French navy had not been a companion in arms like the French army. Did France's interwar naval follies help to keep the Royal Navy from what Marder in 1940 saw as its twin problems since 1880: "The Invasion Bogey" and "The 'Guerre de Course' Nightmare"? On naval aviation, other historians have clear sailing, especially in that landlocked sea that was *Il Mare Nostro* to so many powers.

THEODORE ROPP
Duke University

PAOLA BRUNDU OLLA. *Le origini diplomatiche dell'accordo navale anglo-tedesco del giugno 1935*. (Quaderni della Facoltà di Scienze Politiche dell'Università degli Studi di Cagliari, Serie dell'Istituto di Storia Politica.) Milan: Giuffrè Editore. 1974. Pp. 143. L. 2,200.

"The British authorities, concerned above all with appraising the technical and military aspects of the prospective accord, neglected to examine fully the political implications." Such is the summation of this account of how the British government, with the Admiralty to the fore, so feared unilateral action by Hitler in naval building that they signed the agreement of June 18, 1935, which tolerated German construction up to thirty-five per cent of the tonnage of British Commonwealth fleets. And this less than two months after Britain had subscribed to the condemnation by the Stresa Conference of violations of Versailles.

Here, then, is a study in British foreign policy rather than international affairs. It relies heavily on Foreign Office papers in the Public Record Office, London, a number of which are reproduced—albeit with egregious misprints—in appendixes. Peripheral intelligence is gleaned from the files of the Italian ministry of marine, but on the German side only published documents are

cited. Moreover, Signora Olla is constrained to admit that her sources throw little light on the deeper motives of British policy: did Whitehall gravitate toward Germany out of umbrage at France's overtures to Fascist Italy and the Soviet Union? How far was Britain swayed by the rise of Japan or the looming Ethiopian affair? Skirting these questions, this brief book—text and notes occupy a bare seventy-three pages—confirms more than challenges received opinion.

ALAN CASSELS
McMaster University

SIR GEORGE CATLIN. *For God's Sake, Go! An Autobiography*. Gerrards Cross, Bucks.: Colin Smythe; distrib. by Palm Publishers, Montreal. 1972. Pp. 470. \$14.75.

G. E. G. Catlin, pioneer of American political science, prolific author, and tireless fighter for peace, was born some seventy-eight years ago and educated at St. Paul's and New College, Oxford. He received a doctorate from Cornell and became a professor there in 1925. Newly married to novelist Vera Brittain, the next decade was probably the happiest period of his life. Their daughter, born during this time, is the Labour minister, Shirley Williams. Catlin argued in two early works for an exact science of politics based upon the concept of power and headed a nationwide study of prohibition that contributed to the eventual repeal of the Eighteenth Amendment. Returning to England in 1935, he devoted his life to lecturing, journalism, the analysis of the causes of war and peace, and the promotion of Indian independence and the ideal of "Atlantic Community." He was an unsuccessful perennial Labour candidate for Parliament, advised world figures, and associated with intellectuals in numerous enterprises.

Rambling, overdigressive, and anecdotal, often reading like an international "Who's Who," and badly in need of editing, the book does offer some fascinating sketches of prominent individuals. Although in many respects a singularly impersonal story, it is a useful account of turbulent years from the pen of an egocentric who is deeply embittered by his homeland's failure to reward his talents with a suitable academic appointment or other office. Always an outsider in England, his record tells something of the inside operations of the Establishment. One further value of the autobiography is its clear revelation of the roots of the American science of politics—to which Catlin made a significant contribution—in the Utilitarianism of Hobbes, Bentham, Mill, and the Webbs, and classical political economy, and scientific positivism. Viewed in such a light, this "sci-

ence" of politics seems particularly parochial, naive, and old-fashioned.

NEAL WOOD
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ANTHONY SUTCLIFFE and ROGER SMITH. *Birmingham 1939-1970*. (History of Birmingham, volume 3.) New York: Oxford University Press, for the Birmingham City Council. 1974. Pp. xiv, 514. \$38.50.

In 1952 there appeared a history of Birmingham in two volumes, commissioned by the city council and written by Conrad Gill and Asa Briggs. It is still the best history of any major British city in the modern period, and it did much to make Birmingham the best-known example of British urban development and therefore the inevitable point of comparison for the study of other British towns. By commissioning a third volume to cover the years 1939-70, the city council has again exercised an enlightened patronage. The careful analysis that its two authors, Anthony Sutcliffe and Roger Smith, bring to the recent history of the city places Birmingham yet again in the center of attention for all who wish to reflect on the British postwar urban experience.

The authors have produced a work of high professional standard. Particularly perceptive are the opening and the concluding chapters, where they stand back from their material and reflect on their work. Elsewhere the going is harder and the detail can be overwhelming.

The fundamental question, as they are well aware, is whether Birmingham in the mid-twentieth century was still a suitable unit of study, as it had certainly been in the period covered by the earlier volumes. Since the completion of the work, that unit has in fact been merged into the larger metropolitan county created by the reorganization of English local government in 1974. While this impending demise might strengthen the case for rounding off the history of the city, it also underlines the fact that during these very years there were powerful forces at work, integrating Birmingham into larger economic, social, and political structures.

The book speaks of the declining power of the city to shape its own destiny and emphasizes in a variety of contexts the crucial role of the central government in circumscribing or permitting its development. The government's industrial location policy, while it placed few obstacles in the way of the expansion of the traditional local industries, directed new kinds of industry away from the west Midlands. Since the high cost of land and labor was working in the same direction, however, it is hard to tell how much of the result was in fact the government's doing. The most obvious conflict be-

tween local priorities and central control occurred over boundary extension, a subject over which cities had of course never enjoyed a free hand. The story of the conflicts between the city council, convinced by 1959 of the absolute necessity of acquiring large tracts of new land for public housing, and successive ministers of housing who preferred arrangements by which Birmingham exported its population beyond its boundaries, gives an excellent insight into the relation of local and central government in modern Britain. It also explains better than anything else why the reorganization of local government units had become desperately overdue by the early 1970s.

In dealing with the two most striking features of Birmingham's development in recent years, colored immigration and city-center renewal, the book is also breaking new ground. By recalling the circumstances under which decisions were made that appear in retrospect to have been of crucial importance, it acts as a valuable corrective to much loose talk. It is generously provided with statistical tables and photographic illustrations. Only the lack of a legible map mars what is in other respects a beautifully produced publication.

E. P. HENNOCK
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G. W. S. BARROW, editor. *The Scottish Tradition: Essays in Honour of Ronald Gordon Cant*. [Edinburgh:] Scottish Academic Press; distrib. by Rowman and Littlefield, Totowa, N.J. 1974. Pp. xi, 267. \$16.00.

It is most fitting that a collection of essays should be published to mark the retirement of Ronald Cant, whose geniality and liveliness of thought have long inspired not only his colleagues and students at St. Andrews but many others interested in things Scottish. That this collection of essays should be styled *The Scottish Tradition* is, however, misleading, suggesting, as it does, that some unifying theme is to be expounded. In his introduction the earl of Crawford and Balcarres suggests that "their unity is ultimately founded . . . on that microcosm of Scotland, St. Andrews." But St. Andrews can hardly be regarded as a microcosm of Scotland, and the nineteen essays themselves, whatever they owe in inspiration to St. Andrews, are mostly unrelated to it in content—and, indeed, to one another.

They begin, however, with an essay on St. Andrews before the time of Alexander I. Here the fine scholarship of Majorie Anderson is rendered somewhat sterile by the absence of readily intelligible conclusions. In contrast Barrow not only displays his incomparable skill as an editor of texts in "Some East Fife Documents of the Twelfth and Thirteenth Centuries" but expounds the texts—undramatic though they be. The St. Andrews connection is maintained by Marinell Ash in her

most useful study of Bishop William Lamberton, a major historical figure associated with both William Wallace and Robert Bruce. The remaining essays include a demonstration that Peter's Pence was not levied in Scotland, as well as studies of the architecture of Scottish collegiate churches, funerary monuments, the early history of Scottish printing, a Spanish Armada shipwreck, the legal concept of corporate personality as applied to the Scottish burghs, matriculation patterns of the Scottish universities, the political attitudes of William Drummond of Hawthornden, the reaction of the Swiss Reformed Churches to the early stages of the Covenanted movement, coal mining in Fife in the later eighteenth century, letters of Charles Townshend that are of more than the educational interest suggested by the essay title, an exposition of the muddled administration of the forfeited annexed estates, 1752–84, and a contemplation of the faculty of advocates as possible "ancestors of the modern trade unionists"—a paradoxical exercise that at least gives a fine portrayal of the advocates and the judicial system in which they worked. It might hardly be supposed from the title of the final essay, "Lexicography and Historical Interpretation," that the author is largely concerned with corn-drying kilns.

Given the present—and highly desirable—tendency to regard all aspects of human experience as fit subjects for the historian, it might be invidious to single out any particular essay as being especially valuable. Yet this distinction may reasonably be claimed for David Murison's essay, "Linguistic Relationships in Medieval Scotland," in which, for the first time, the linguistic basis of the cleavage between Highlanders and Lowlanders is fully and authoritatively expounded. Apart from this, and a few other exceptions, the essays are somewhat esoteric. Indeed in "The Illusory Breve Testatum" J. J. Robertson candidly admits that his speculation may be "insignificant, unpromising and barren." The lucidity and learning that he displays must, however, disarm criticism, a consideration applicable to the whole book, which exemplifies high standards of scholarship.

This is not a book for those who come to Scottish history for the first time. It is decidedly for the *cognoscenti*. Having already trodden the broad highways of Scottish history they may all the better appreciate the scenery along the less-frequented paths that it has marked out.

RANALD NICHOLSON
University of Guelph

MARK DILWORTH. *The Scots in Franconia: A Century of Monastic Life*. Totowa, N.J.: Rowman and Littlefield. 1974. Pp. 301. \$13.50.

These Scots are not an ethnic minority or a hitherto unmarked migration, but are rather the for-

mer Benedictine monks of St. James in Würzburg. Nor is the book a monastic variant on the village-history form, for there is almost no account of the internal life or economy of the monastery. Devotional activities are hard to find out about and do not really interest Father Dilworth much anyway. The origins of the monks, aside from their being Scottish Catholics, are hazy and scarcely noted. Even the most earnest local historian might be baffled at what to say about a community that rarely numbered more than three members at one time and apparently peaked at twelve.

One might begin by asking how so small and eccentric a foundation survived during the religious turmoil of the sixteenth and seventeenth centuries. This is where Dilworth's narrative mainly falls. By the mid-sixteenth century the monastery's medieval foundation had decayed and become a sinecure for ecclesiastical officials without serious monastic interests. But around 1595 the monastery was revived by the energetic prince-bishop of Würzburg, Julius Fechter, as a center to recruit and train young Scots for missionary work back in Scotland. Its subsequent history gives a captivating everyday view of the European Counter Reformation politics that kept the monastery intact. The Vatican and the bishops of Würzburg fended off repeated efforts by German establishments and secular clergy, by Jesuits, Spanish Carmelites, and even by the emperor to take control of Scottish monastic revenues for more parochial purposes.

Dilworth's research was careful, his judgment discriminating, and his presentation is fluent and invites confidence; he has a lively imagination with a nice sense of limits. Two items from the supernatural history of the monastery must be mentioned. The founder and house saint of St. James, the abstemious twelfth-century Abbot Macarius, was famed for his miraculous ability to turn wine into water and so represented in chronicle and portraiture. A second miracle involved the dispatch of certain relics of Macarius to England in 1688, at the Queen's request. These were to succor the newborn Prince of Wales, who was at the time expected to die of the same ailments that had carried off his five elder siblings. The relics were appropriately applied, the infant recovered, and the Glorious Revolution ensued. The Old Pretender is said to have carried the relics about with him ever after, but with apparently limited effect. Readers of the *AHR* will need no special notice of the possible implications of these events.

MACK WALKER
Johns Hopkins University

DAVID STEVENSON. *The Scottish Revolution, 1637-1644: The Triumph of the Covenanters*. New York: St. Martin's Press. 1974. Pp. 416. \$15.95.

Despite Gilbert Burnet's observation that no clear understanding of the Great Rebellion can be achieved without first fully stating its Scottish origins, historians of the twentieth century have proceeded to reassess the English Civil War and compare it to Continental conflicts with scarcely a backward glance toward the northern kingdom. To correct this omission, David Stevenson has written a political history of the Scottish people from the beginning of the agitation against Charles I's religious policies through the temporary constitutional triumph of the Covenanters to their military intervention south of the Tweed. Building upon the standard interpretation found in Gordon Donaldson's *Scotland: James V to James VII* (1965), the author stresses the importance of secular motives and laymen working through the constitutional framework. While not denying the importance of ecclesiastical disaffection, Stevenson subscribes to G. R. Elton's dictum that "political history must come before any other."

What results is a detailed elucidation of the merged secular and religious ground swell of Scottish nationalism leading to the Solemn League and Covenant. Covenanter was not an exclusively clerical label. This elaboration on the more familiar church history of the period provides a fresh re-emphasis. The author demonstrates that the Covenanters were seeking a closer union with the English Parliament for such mundane reasons as securing commercial benefits, expanding Scottish influence abroad, and curbing the excessive royal power. His work is thorough and insightful, but the purported balance is strained. While admirably expounding political concerns, he underplays the importance of such religious matters as ecclesiastical patronage, the liturgy, and governance of the kirk—concerns that lay at the root of the quarrel with the Stuart monarchy.

The title of this expanded doctoral thesis is a misnomer. As important as these events were for Scotland's future, they were achieved without overthrowing the constitution. Stevenson carefully accounts for the rise of the Covenanters to political and military power, but he neither demonstrates that their triumph constituted a revolution nor assesses the total and rapid collapse of their movement. A final chapter assumes the existence of a revolution and then provides a brief analysis of the dimensions of that rebellion and attempts to draw comparisons with more recognizable seventeenth-century revolutions. Yet for the Scots nearly half a century would pass before they precipitated another conflict that proved to be revolutionary in its effect upon church and state.

In reaching his conclusions, Stevenson has utilized the standard Scottish manuscript and secondary sources. A bibliography, index, and maps are included. The inappropriate title and somewhat

graceless style of this chronological narrative are unfortunate but should not restrict the usefulness of this welcomed addition to seventeenth-century Scottish constitutional history. One hopes that it will stimulate further research in a field that too long has been neglected.

ROBERT PAUL BARNES
Maryville, Missouri

BRENDAN BRADSHAW. *The Dissolution of the Religious Orders in Ireland under Henry VIII*. New York: Cambridge University Press. 1974. Pp. viii, 276. \$16.50.

Early in the sixteenth century, the Irish Church was in a sorry state and monasticism was crumbling from within, but the mendicant orders were building upon recent decades of vigorous expansion. The various phases of suppression, as they developed from 1537, would in consequence have to deal with widely differing communities. Most crucial was the approach of St. Leger, who became lord deputy in 1540. He anticipated that suppression and the resulting transfer of land, particularly in a period of rapidly changing rules of land tenure and descent, would serve as a tool for reconciliation, the acceptance of the Crown, and the establishment of a new relationship between Dublin and the localities. Initially confined, this dual process became national in 1541 when Henry became king. St. Leger's concern for political equilibrium, however, largely explains the slow progress of the movement. When Henry died a significant percentage of monastic foundations and mendicant communities survived. The Reformation was never really forced upon Ireland in the central Tudor period, but eventually that country would represent an outstanding success for the Counter Reformation. This had little to do with the resilience of friars and monks. Instead, by this account, liberal and constitutional nationalism were in the air until 1557. It was colonization, not conciliation, which fastened upon the transfer of land; resistance looked beyond the British Isles, and Ireland acquired its "ideological neurosis."

This coherent and explicitly constructed monograph explores an Irish story rooted in native demographic and political considerations. In his provision of details and analysis of episodes, Bradshaw has not only illuminated a subject crying for its historian but contributed to our burgeoning sense of Irish history. Only the price is to be regretted.

W. J. JONES
University of Alberta

BRIAN FARRELL, editor. *The Irish Parliamentary Tradition*. With three essays on the Treaty debate by F. S. L. LYONS. New York: Barnes and Noble. 1973. Pp. 286. \$11.00.

These nineteen essays, which effectively blend matters of general interest with scholarship, begin and

end with the assertion by the editor, Brian Farrell, of the importance of the Irish parliamentary tradition and come at a time when assertion is needed. Revolutionaries and secret societies easily capture the imagination of the general reader, giving the impression that Irish history in the last two centuries has been shaped primarily by conspiracies, uprisings, and martyrdom and obscuring the role of the Grattans, O'Connells, and Parnells who, during most and possibly all periods of recent Irish history, have enjoyed the sympathies of the majority of the people.

The essays on the early Irish parliaments, which include one on representative institutions in Gaelic Ireland, make the point that even in its medieval context, the Irish parliamentary tradition owes much to the connection with England. Hugh Kearney, in his excellent essay on seventeenth-century parliaments, finds that they had little influence on later developments but makes it clear that they are of interest for their own sake. The first half of the book is devoted to the history of the parliamentary tradition up to the eighteenth century, which is reasonable in a volume of some 250 pages, but more space might have been given to Joseph Lee for his treatment of Grattan and to Oliver MacDonagh's essay on O'Connell. Kevin Nowlan provides a brief but clear explanation of the eclipse of parliamentary influence during the famine.

The power of Irish parliamentarians from Grattan to Parnell was in their ability to make use of "out-of-doors" influence. This was always difficult, but their achievements were more impressive than those of the United Irishmen, the rebels of '48, and the Fenians, who broke with the parliamentary tradition. The Easter Rising of 1916 was in defiance of Irish parliamentarians as well as of British power. It brought about a new balance of forces in which the "out-of-doors" agitators, who could be managed by parliamentarians, were replaced by a partnership with a military underground movement that the parliamentarians could not control. The essays end, in a sense, on a note of failure in F. S. L. Lyons's perceptive discussion of the treaty debate. With independence virtually established, the losing party in the debate sought redress in civil war.

This is an excellent and timely volume. It deserves attention because contemporary events in Ulster have obscured the fact that, although shaken on occasion, the parliamentary tradition has been and probably still is the strongest force in Irish politics.

HEREWARD SENIOR
McGill University

E. D. STEELE. *Irish Land and British Politics: Tenant-Right and Nationality, 1865-1870*. New York: Cambridge University Press. 1974. Pp. ix, 367. \$19.50.

People working in Irish history emphasize the relationships between agrarian discontent and agitation and the growth of Irish nationalism, but until the recent contributions of scholars like Joseph Lee, James Donnelly, Jr., and Barbara Solow, the actual economic and social conditions of nineteenth-century agrarian Ireland were shrouded in the fogs of nationalist mythology and propaganda. In *The Land Question and the Irish Economy, 1870-1903* (1971), Solow presented evidence indicating that in the 1850s, 1860s, and early 1870s, the standard of living in rural Ireland improved considerably as prices increased more than rents. She argued that Irish agrarian radicalism was more political and social than economic and that Irish land acts passed by the British Parliament responded to political and social demands rather than economic needs.

In his analysis of the Irish agitation, British public opinion, and British political dimensions of the Land Act of 1870, E. D. Steele supports Solow's theses. Gladstone understood that Irish nationalism fed on peasant resentment against landlords as representatives of alien conquest and occupation. He tried to give tenant farmers a stake in the land they occupied by legislating and extending Ulster custom throughout the country. The fears of the British establishment, however, expressed by journalists, pamphleteers, and politicians from both major parties, that tenant right in Ireland might undermine property rights all over the United Kingdom, forced the prime minister to retreat from fixity to security of tenure. The Land Act hoped to achieve the latter by legislating custom where it existed and by preventing evictions in other places through compensation for improvement and disturbance. The Bright clause encouraged land purchase and peasant proprietorship on a modest scale.

Irish agrarian and nationalist agitators rejected the Land Act of 1870 as an inadequate response to their demands for fixity of tenure, and the vicious cycle of violence and coercion continued. But Gladstone's efforts pioneered the way that finally led to peasant proprietorship, and his attempt at agrarian reform in Ireland established a precedent limiting property rights in the United Kingdom, opening the gates of legislation that eventually produced the welfare state.

Although in his background discussion of the Land Act of 1870 Steele tends to exaggerate the agrarian radicalism content of Irish nationalism, particularly in its Fenian manifestation, and toward the end of his work he obscures the Irish dimension in the intricacies of British political and journalist debate, he has contributed a valuable book. His thorough research and intelligent analysis have clarified the complicated interactions between Irish agitations and British politics. But a

shorter book would have been better and less expensive. Repetitive details and arguments dull rather than illuminate issues and often make for tedious reading.

LAWRENCE J. MCCAFFREY
Loyola University,
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DOMINIC DALY. *The Young Douglas Hyde: The Dawn of the Irish Revolution and Renaissance, 1874-1893*. Foreword by ERSKINE CHILDERS. Totowa, N.J.: Rowman and Littlefield. 1974. Pp. xix, 232. \$13.50.

"When the Gaelic League was founded in 1893," wrote Patrick Pearse, "the Irish Revolution began." Its founder, Douglas Hyde, was then thirty-three. Apart from a short biography by Diarmid Coffey in 1938, little has been known about his personal life. Dominic Daly has had access to thirteen volumes of a diary kept regularly by Hyde between 1874 and 1912, and on this basis he has reconstructed Hyde's life down to the founding of the league. We learn in fascinating detail how as a boy of fourteen Hyde learned Irish from a gamekeeper and a poor countrywoman. We also learn how his passion for Irish led him to acquire what Daly calls "an extraordinary library for a young man of twenty who never had a formal lesson in Irish history or literature." More surprising is the revelation of the bitter hostility that existed between Hyde and his father, a parson, and of the open quarrels that took place. Hyde once wrote that his father was "the most opposed and obnoxious to my way of thinking that could ever exist on the face of the earth."

In view of the fact that Hyde strenuously resisted all attempts to involve the Gaelic League actively in politics, it is surprising to learn how ardent was his Fenianism and how strong was his hatred of England as a young man. "My hundred thousand curses on England and on her rule," he wrote in 1881. No less strange is the almost total absence from the diary of any mention of Parnell, though Tim Healy is referred to once as "a scoundrel."

Though Hyde was elected president of the Gaelic League and remained so for twenty-two years (1893-1915), Daly makes it clear that Eoin MacNeill was chiefly responsible for establishing the new organization. Nevertheless Hyde gave up his own creative work to become the apostle of the league, leading Yeats to speak of him regretfully as "the great poet who died in his youth." Daly's book is indispensable to students of the Irish Literary Revival.

GIOVANNI COSTIGAN
University of Washington

I. D. MCFARLANE. *A Literary History of France: Renaissance France, 1470-1589*. New York: Barnes and Noble. 1974. Pp. xxiv, 557. \$18.00.

This volume, in a series under the general editorship of P. E. Charvet, does not escape all the pitfalls of survey treatments, yet has much to offer. I. D. McFarlane, professor of French literature at Oxford, provides a sensitive, informed commentary on scores of writers between 1470 and 1587, a time long recognized as one of the most vigorous and tumultuous periods of French literature. Recognizing the importance of the historical setting, McFarlane has attempted to describe the social and political context in the general introduction and in sections of individual chapters. These sections may attempt too much. For the specialist they often appear insufficiently nuanced; yet they do include many qualifications, and the average student will probably find them too thick with underbrush. Such are the frustrations of attempting to cover a rich century in five hundred pages.

It is when McFarlane takes up the individual writers that his experience as a *don* pays off. He guides the reader through the century, phase by phase, introducing the major writers and many minor ones as well. He seems to have kept abreast of the flood of secondary studies on François Rabelais, Pierre de Ronsard, and Michel Eyquem Montaigne, but he is careful to keep his balance. After noticing the tendency of the present generation to emphasize Rabelais's Christian orthodoxy, McFarlane suggests a recent "shift of emphasis towards the study of Rabelais's art and the nature of his comic genius." Such comments do not, of course, take the place of commenting on the texts themselves, and in this McFarlane is superb. He usually manages to include at least a stanza or paragraph. In explicating these passages he is perceptive and sensible, alert to both style and substance. His broad background in French literature enables him to make illuminating comparisons with more recent writers like Stéphane Mallarmé, Paul Valéry, Paul Claudel, and Gabriel Marcel.

Quite willing to include his personal evaluations, McFarlane makes a rather convincing case for elevating the significance of some writers on the second level. In *Délie* (1544), for example, Maurice Scève "puts his stamp on the evolution of love-poetry for the rest of the century." And Agrippa d'Aubigné, admittedly capable of a tedious moralism, is praised as one whose significance "transcends national boundaries." Incidentally, among McFarlane's previous works are editions of *Délie* (1966) and d'Aubigné's *Tragiques* (1970).

The author is not merely a literary historian but also a historian of ideas. Both his chapters and his bibliographical references indicate a strong interest in comparative literature. Classical and Italian

influences are frequently mentioned. Even more valuable is McFarlane's clear recognition of the polemical atmosphere of the century. He carefully avoids isolating ideas and taking them as representative of the period, when in fact they can be properly understood only in relation to their polarities. "There is no *leitmotiv* of Renaissance man that does not immediately conjure up its counterpart," he writes, "and it is no coincidence that, at a time when these polarities are exacerbated in the context of a dislocated world-picture, the Neoplatonic theme of unity in diversity should command so much sympathy."

There are minor annoyances, however. The glossary, while adequate for most purposes, includes some definitions that are surprisingly imprecise. A general bibliography is mainly limited to book-length studies, omitting all but a handful of articles. The section "Further Reading and Study" dismisses America as needing a separate description because of its "immense resources." But such petty irritants do not limit the value of a book that contains much solid nourishment.

DAVIS BITTON

University of Utah

PIERRE GOUBERT. *L'Ancien Régime*. Volume 2, *Les pouvoirs*. Paris: Armand Colin. 1973. Pp. 262.

Volume 1 of Goubert's *Ancien Régime: La société* (1969), the best short introduction to French demographic and social history, 1560-1789, was followed in 1973 by the second and final volume, subtitled *Les pouvoirs*, which in my opinion is the best short introduction to institutional and political history for the same period. Not everyone will agree with this assessment of *Les pouvoirs*, largely because Goubert simply omits what many specialists of the *ancien régime* in English-speaking countries still believe to be institutional and political history. Goubert ignores almost completely the narrative of court politics and ministers-parlement conflicts in order to suggest in the boldest possible terms that this traditional narrative is the mere tip of the iceberg in the broader *histoire totale* of the "powers."

Together these volumes constitute a somewhat truncated study of the *ancien régime* established by the leaders of the so-called *Annales* school. Goubert's organization, his views on economic and social structures, and particularly his stress upon continuities immediately suggest comparing this book with Marc Bloch's *Feudal Society* (1961) and Fernand Braudel's *The Mediterranean and the Mediterranean World in the Age of Philip II* (1972). There are differences, but the similarities, particularly that studied lack of precision in defining institutions and the looseness between the social and

institutional structures of society, are what one expects from members of the *Annales* school. The truncation in this instance is not one of form but, rather, of content; Goubert presents a major work of synthesis while providing very little evidence or examples to support his argument. Instead, the form is that of a witty textbook almost without notes, but with selected source materials excerpted and published without comment. A historian less able than Goubert, or more inhibited by the professional conventions of historians or worrisome textbook editors, might have produced a disaster, but his almost complete control of both old and new monographic literature and *annaliste* problematics about social organization turn this book into a tour de force that merits the attention not only of specialists of French history but of history in general.

Goubert defines the "powers" of French society as neither narrowly derived from social structure nor derived simply from institutions. They are only greater and lesser capacities for coercive action; the king, royal ministers, councils, law courts, bishops, nobles, bourgeois, Protestants, *curés*, tax collectors, and so forth are all endowed with greater or lesser "powers" in the *ancien régime*. According to Goubert the aim, if not the principal function, of all the powers was the extraction of wealth from the least powerful and powerless elements of society. Those invested with "powers," either as institutions or as individuals and social groups, had proportionately less power to extract wealth from others wielding power than from the powerless; thus they together collected rents, taxes, seigneurial dues, tithes, and so forth from the millions of peasants and artisans who were the producers of wealth in this traditional economy. "Powers" are thus, in a sense, not only the ability to coerce others to give up their wealth, but also the ability of the powerful to avoid having to give up wealth.

Goubert implies that the narrative of state efforts to alter the "powers" need not be given because not even the most powerful of kings and ministers altered in any fundamental way this equilibrium of "powers" that characterized the *ancien régime*. There were peasant revolts, of course, that were brought on by the conjuncture of crop failure and a demographic crisis which coincided in the 1630s and 1640s with the "powers'" efforts to raise taxes; yet it was not the increased taxation but rather the agrarian and demographic crises that forced the peasants to pay a heavier toll than anything demanded of them by the state. Popular revolts, like Colbert's tax reforms, failed to alter the fundamental equilibrium of powers in a kind of *société bloquée* that was the *ancien régime*. The determinants of political action are social and institutional throughout the book; individuals, just

as in Bloch and Braudel, are convincingly portrayed only enough to define their extremely narrow and almost inconsequential spheres of action.

The most provocative aspect of the book is the concluding analysis of the *ancien régime* itself. Existing in time, as history, it of course collapsed in 1789, but Goubert boldly asserts that many patterns of social behavior, notably about paying taxes, survived the French Revolution and continue to be influential at present. And when the leading historical demographer of France trenchantly concludes that it was higher average age of life after 1750 that accounts for the rise in population, the result is a bold new interpretation of the French Revolution. While expressing cautious acceptance of all the conclusions about the social and economic causes of 1789, Goubert eagerly stresses the effects of a *surcharge de jeunes*. With older generations living on an extra decade, there simply were not enough housing, jobs, and royal offices all up and down the society to permit the young to have the lives and careers that they had been raised to expect. This monumental delay in marriage and placement led to the *montée des jeunes* of 1789. Here Goubert's assertions will need further testing and refinement, but this newly emphasized cause of the Revolution ought not to be dismissed as merely a reading of the "events of 1968" in France back into those of 1789. Goubert stresses the solidity and continuity of the *ancien régime*; he must have more than the political history of elites, the aristocratic and bourgeois revolutions to cause the collapse of such a strong structure of "powers." Historians may find it useful to pursue Goubert's assertions and to begin exploring the effects of aging and growing populations on strained status structures and institutions.

OREST RANUM

Johns Hopkins University

ROLAND MOUSNIER. *Les institutions de la France sous la monarchie absolue, 1598-1789*. Volume 1, *Société et État*. (Histoire des institutions.) Paris: Presses Universitaires de France. 1974. Pp. 386.

When a recognized historian of the highest rank publishes a massive work of synthesis during the latter part of his career the event assumes major significance for his professional audience. Such a work Professor Mousnier here presents in this first volume of his elaborate treatment of French institutions during the age of absolutism. The undoubted competence of the author and the lack of anything comparable for this period of French history combine to render the book uniquely valuable to all students of the period. Eschewing earlier narrow approaches to institutional history, Mousnier defines an institution merely as any human

collectivity that is animated by a given objective and ideal, occupies a definable position in society, functions according to known procedures, and makes a contribution to the whole. Furthermore, he reflects the influence of recent French scholarship by insisting that such institutions cannot be understood apart from the broader social, economic, and ideological structures that prevailed in their time and sharply influenced the lives of all persons and groups. He therefore devotes all but the final two chapters of this massive volume to an elaborate analysis of French society and then moves from these social foundations to a discussion of the monarchical system of government. The very extensive specifics, with which he treats all topics, reflect both his vast personal knowledge of legal, institutional, social, economic, and intellectual history—much of it evidenced in his earlier publications—and the latest French scholarship. Not the least valuable feature of the book is its extensive bibliographies at the end of each section.

A mere listing of the contents of the volume will indicate its comprehensive coverage. After recounting the evolution of views concerning French society by comparing the ideas of Loyseau, Saint-Simon, Domat, and Barnave and analyzing the enduring significance of the ties that stemmed from family relationships and personal fidelities, Mousnier takes up the elements of what he calls the "society of orders." First the nobility, with its gradations, privileges, social status, economic position, and ideology, is examined in detail with copious supporting information. An interesting subsection is devoted to the *anoblis* and the *bourgeois vivant noblement*. Less detailed is the treatment of the commonality: bourgeoisie, manual laborers, and the poor. The extensive section on the clergy includes not only all customary items—its elements, privileges, wealth, social origins, institutions, and Gallicanism—but also its style of life, and the rise and decline of the Catholic Renaissance. The Protestants and Jews as elements of society are also examined. This section is followed by an analysis of both the *société de corps* (groupings of officials, jurists, universities, academies, and guilds), which reflects the influence of Olivier-Martin, and the seigneurial system, which includes the usual economic and judicial factors and a suggestive treatment of the seigneurial reaction in the eighteenth century. Villages, parishes, cities, and provinces are analyzed for their institutions that combined territorial and corporate features. Finally the author turns to the monarchical state, the governing power that was indispensable for preserving order in this thoroughly fragmented society. Here the nature, power, and ideology of absolute monarchy are treated, together with an

estimate of its very limited human and financial resources.

The resulting work takes the form of an elaborate, authoritative manual and is in large measure a very scholarly objective compilation. Inevitably, however, Mousnier incorporated some of his personal views and preferences when analyzing so vast a subject. One appears in his insistence that religious conceptions and values were fundamental to the structure, functioning, and purposes of the society of orders that prevailed in seventeenth-century France. The significance of this factor he makes clear, although it is unusual in treatments on this subject. Another is his repeated contention that during the eighteenth century, French society evolved from a society of orders to a society of classes. The first was religious, hierarchical, authoritarian, and static, with traditional institutions and values to which all conformed, whereas the second was secular, functional, democratic, and dynamic, with the people and their interests ultimately in control. He repeatedly finds evidence of this transition in the changing attitudes and social alterations of the later period, and, indeed, insists that a major purpose of the work is to trace this fundamental reshaping of French society and institutions. His personal preference enters notably in his evident distaste for this evolution, and he does not hesitate to label it a change from an *état d'ordres* to an *état-gendarme*. This personal note, however, does nothing to lessen the value of the work. It will long serve as an extremely valuable synthesis for its vast information and many suggestions for further research.

WILLIAM F. CHURCH
Brown University

DOROTHY ANNE LIOT BACKER. *Precious Women*. New York: Basic Books. 1974. Pp. xi, 308. \$10.95.

The seductive intimacy of the seventeenth-century *précieuse* alcove is expertly evoked in this study of a phenomenon more often ridiculed than understood. Would-be prudes sharing secret metaphors with sighing suitors, elaborate discussions of "questions d'amour," an adored mistress reigning over all from her bed-throne—these major components of life *à la précieuse* seem frivolous, but, according to Backer, they hid a deeper purpose. *Préciosité*, she argues, was a "feminist" phenomenon, an attempt to free women from early marriage—or failing that, the convent—and repeated pregnancies. To achieve this freedom, the *précieuse* retired to her bed with real, feigned, or psychosomatic illness. From there she received her friends and admirers, entertained lavishly, practiced conspicuous prudery, and enjoyed literary occupations of dubious merit. The *précieuse* movement,

launched by the marquise de Rambouillet during the age of Richelieu and interrupted by the Fronde, became "bourgeois" in the 1650s, went out of fashion, and was dealt the *coup de grâce* by Molière's satire in 1659. The women who survived gave themselves a new name, "femmes savantes," but these too suffered ridicule by the same satirist. Ultimately, intelligent educated women, inspired by the popularization of science that announced the Enlightenment, turned from literature to physics as their main preoccupation. They also abandoned their alcoves for the salons and in general were more interesting than their predecessors.

This book on the precious women suffers from a lack of compelling intellectual material in *précieuse* life. After a promising beginning—a re-creation of an alcove setting—the book offers little more than a string of female portraits, endless anecdotes of unfulfilled love, private intrigue and verbal duels, and recapitulations of poor poetry and bad novels. Even the author, while sympathizing with the efforts of the *précieuses*, writes ruefully of their low level of accomplishment. Indeed, to have earned the title "feminist phenomenon," the *précieuse* would have had to fight for social, political, economic, and legal equality with men. Not only did she not do this, but men—pleasing them, teasing them, and winning them—remained the center of her life. The movement was a "feminine" phenomenon; created by and for women, it had a considerable effect on the "feminization" of French social habits. Between the uncouth age of Henri IV and the smooth etiquette of Louis XIV stands the marquise de Rambouillet's "chambre bleue" and her real and spiritual daughters.

ELISABETH ISRAELS PERRY
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A. J. KRAILSHEIMER. *Armand-Jean de Rancé, Abbot of La Trappe: His Influence in the Cloister and the World*. New York: Oxford University Press, 1974. Pp. xvi, 376, 8 plates. \$25.00.

Scholars and teachers sometimes tend to forget that the seventeenth century in France was an age of revitalized faith, the flowering of the Counter Reformation. Alongside famous administrators, warriors, and intellectuals one must place the famous saints and obscure religious men and women as true expressions of French creativity and genius. Such a man was Armand-Jean le Bouthillier de Rancé, the abbot of the reformed Cistercian abbey of La Trappe.

Rancé's career is known mostly from Chateaubriand's romantic *Vie de Rancé* (1844) and Henri Bremond's unreliable *L'Abbé Tempête* (1929), nei-

ther of which can be deemed objective biography. Rancé's contemporaries seemed to have regarded him as either a saint or a charlatan, and early biographies all obscured or concealed much valuable material about his life before and after his entry into the cloister. Mr. Krailsheimer is clearly sympathetic toward his subject and desirous of defending him from charges of excessive austerity and combativeness. This book is not a biography of Rancé. It is a presentation of all the available evidence in manuscript and printed sources, the result of extensive research in numerous archives in a number of countries, and is designed to let the historical figure of Rancé emerge from behind legend and distortion. As such the book is not completely digested or digestible and makes difficult reading, but the author's method and purpose are in the best tradition of scientific scholarship. Krailsheimer has no particular thesis to advance; he offers the materials available for a reassessment of the career of Rancé, a man who managed to take in hand a decayed Cistercian community and, against strong opposition, to create a flourishing community of austere monks, whose life of prayer and manual labor, practiced with severe deprivation, endured in health up to the Revolution of 1789 and still endures in many parts of the modern world. While Rancé's belief in extreme austerity and his prohibition of all theological or intellectual study by the monks of La Trappe may elicit little sympathy today, they were based on deep spiritual understanding. In any comparison with the Jansenists, Rancé's community emerges with heightened stature. Port-Royal had been before its reform a Cistercian establishment, and Rancé shared with the Jansenists a firm Augustinian theology, a distrust of Jesuit casuistry, and a desire to withdraw from a sinful world. But he did sign the *Formulaire* against the five propositions of Jansenius as a matter of Catholic obedience, refusing to place his own conscience above the decision of the Church. Indeed in his attitude toward the Jansenists, with whom he shared so much, he displayed perhaps his most commendable trait—refusal to engage in doctrinal controversies detrimental to individual piety or to the good of the Church. Jansenists never forgave his comment on the death of Antoine Arnauld in 1694, but we may applaud it: "Enfin, voilà M. Arnauld mort. . . . Quoi qu'on dise, voilà bien des questions finies: son érudition et son autorité étaient d'un grand poids pour le parti [janséniste]. Heureux qui n'en a point d'autre que celui de Jésus-Christ." And on the excesses of the Quietists he remarked, "Dieu nous commande de l'aimer, mais non pas de sentir que nous l'aimons."

Krailsheimer has, then, provided materials for a proper appreciation of the genius of Rancé. We

come to understand Rancé's attraction for numerous religious and laymen in the France of Louis XIV, from obscure monks and nuns to fashionable ladies and gentlemen, including such public figures as the cardinal de Retz, the maréchal de Bellefonds, and the refugee James II. One may perhaps compare him to St.-François de Sales, a man of quite different temperament and genius, in this respect: with de Genève, Rancé shunned unseemly argumentation about grace and predestination and believed it more worthwhile to endeavor to make good use of grace than to argue about it. Seventeenth-century scholars will be very much in Krailsheimer's debt for this piece of scholarship.

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RENÉ MOULINAS. *L'imprimerie, la librairie et la presse à Avignon au XVIII^e siècle*. Preface by P. GUIRAL. (Collection "Editorial.") [Grenoble:] Presses Universitaires de Grenoble. 1974. Pp. 441. 75 fr.

From 1740 to 1770 Avignon emerged from relative obscurity to become a major source for the European book trade. Denied their livelihood by the monopolistic practices of the Paris publishing syndicate, several printers and sellers from the French provinces settled in the papal enclave. Their ranks were augmented by native artisans and merchants who forsook their customary occupations in order to manufacture and sell pirated editions. A conjunction of fortuitous circumstances determined Avignon's fortune: except for the occupation of 1768-74, the city lay outside French legal and corporate jurisdiction; and unprecedented demand for French-language books swept across Europe; the Paris publishing gild was ill-equipped to supply the needs of the enlarged readership; and the regime of Louis XV tacitly cooperated in the contraband trade. Though the French occupation of Avignon halted the climb to prosperity for the city's bookmen, it was not until 1777 that the publishing industry there slid into decline. In that year Versailles tried to revive the pitiful state of printing in the French provinces by placing a significant number of titles, formerly monopolized by Paris publishers, in the public domain. French police went after Avignon's contraband with vengeance. In Languedoc and Provence printers, turned distributors for Avignon's production, re-activated their own shops, and their onetime suppliers began withdrawing from the newly perilous industry. In 1785 the survivors of the Avignonese book trade obtained an agreement from the French government that placed a reduced number of them on a par with publishers in the provinces. Thereafter

they would be subjected to the same regulations and restrictions as the printers and sellers of Nîmes, Marseilles, and Lyons. Literary piracy became a crime in Avignon, and the city thereby lost its rationale as a publishing center.

Such a bald summary of Moulinas's splendid pioneer study fails to do it justice. The author touches upon nearly all the issues unearthed by *livre et société* historians in recent years and, without strain, applies them to his work. His chapters on the *Courrier d'Avignon* are exemplary, the best modern account of a topic in the history of pre-revolutionary French-language journalism that I have read. Moulinas has no use for jargon. He skillfully employs notarial records and a few precious stock inventories, and, in refreshing fashion, refrains from connecting too closely his findings on the economic development of Avignon's book trade with E. Labrousse's celebrated hills and valleys. I should like to see Moulinas nuance his remarks on the influence of the French legislation of 1777 on book privileges and would perhaps take him to task for neglecting the economic effects of Vergennes's order of June 12, 1783, to the farmer-general demanding the transport of foreign book imports to the capital prior to reshipment. But these are the quibbles of the specialist. A work of far greater breadth than its title suggests, Moulinas's *thèse* is a superb study in the social history of ideas.

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KENNEDY F. ROCHE. *Rousseau: Stoic & Romantic*. London: Methuen and Company; distrib. by Barnes and Noble, New York. 1974. Pp. xvi, 177. \$11.00.

The title of this book is entirely misleading. Mr. Roche says little about Rousseau's relations to Stoicism and nothing about romanticism. One would, however, have difficulties in finding any suitable title for a book that has no theme at all. It begins with a brief and superficial review of Seneca, but shows no grasp of stoic metaphysics generally. The only point that comes out clearly is that the doctrine of the unreality of evil caused problems. Stoics took human suffering to be due to a perpetual decline from a Golden Age, a fatality for which no adequate cause was given. Rousseau's *Second Discourse* resembles Stoicism in this respect. That is all we hear about the latter. From there on no discernible principle of selection, analysis, or criticism appears. Rousseau's romanticism is, in passing, identified with a belief in the uniqueness of the individual, with nationalism, and finally, with a belief in a future utopia, but none of these topics is discussed adequately. Instead of a coherent view of Rousseau's theories, we

get a very simple account of some, by no means all, chapters of the *Social Contract*. This quasi narrative is enlivened by attempts at "comparing and contrasting" Rousseau with various other writers. William Godwin, Thomas Paine, Jeremy Bentham, Machiavelli, and, occasionally, the Christian tradition. The Stoics, however, do not reappear, even in the chapter devoted to natural rights. At no time are any conclusions drawn from these exercises, and it is impossible to guess what end they are meant to serve. The book ends with the wholly unargued conclusion that Rousseau transformed Stoicism, and that this was possible because the latter was a naturalistic system. This is not a serious work of scholarship.

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JOHN CHARVET. *The Social Problem in the Philosophy of Rousseau*. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1974. Pp. vii, 150. \$8.95.

BRONISŁAW BACZKO. *Rousseau: Solitude et communauté*. Translated from the Polish by CLAIRE BRENDHELL-LAMHOUT. (École Pratique des Hautes Études—Sorbonne. VI^e Section: Sciences économiques et sociales. Centre de recherches historiques. Civilisations et sociétés 30.) Paris: Mouton. 1974. Pp. 420. 72 fr.

These two works represent in their respective perspectives and methodologies opposite ends of the interpretive spectrum. Rousseau's *oeuvre* has generated among scholars. John Charvet limits himself to a critical analysis of the "social problem" in Rousseau's philosophy, that is, of the individual's relations to others in society. He concentrates on but three of Rousseau's works—the *Discourse on Inequality*, *Emile*, and the *Social Contract*. They are, Charvet writes, "the major statements of his mature thought on the subject"; and he eschews any reference to other interpretations or analyses. His, then, is a microscopic approach to Rousseau. Bronisław Baczko's approach, in contrast, is macroscopic. It encompasses not only Rousseau's entire corpus but most of the major interpretive works on Rousseau in Polish, German, French, and English. Neither a biography of Rousseau nor a chronological analysis of his works, Baczko's study is "an attempt to extricate Rousseau's themes, to grasp the unity of his vision of the world imbedded in his works not so much as a system of theses and solutions but rather as a structure of tensions and problems." "Solitude" and "community" are for Baczko the key themes in Rousseau's work, reflecting and elaborating the tensions and critical problems Rousseau experienced and sought to resolve. Inasmuch as Baczko's con-

comitant aim is to "reconstitute the most general problems of the age of Enlightenment" in their dynamic unfolding and as announcing the "crisis" of Enlightenment thought, Baczko necessarily adopts a broad, not to say exhaustive methodological perspective.

Charvet's narrow approach allows for brevity and clarity, both of which adorn his essay. His concern is with the nature and logic of Rousseau's conception of a society and of social relations between men in which the corruption Rousseau associates with society is avoided and human potentialities for virtue are fully developed. He traces the unfolding of Rousseau's argument and exposes the basic philosophical difficulties inherent in it. He analyzes the *Discourse on Inequality* as "the preliminary presentation and analysis of the social problem," the work in which Rousseau describes how man's fall from nature into society creates a fundamental problem as to individual identities. He then analyzes *Emile* and the *Social Contract*, Rousseau's accounts, respectively, of the educational and moral relations and of the political relations that must be realized if a solution to the social problem is to emerge. Charvet persuasively argues that Rousseau's thought is "incoherent." He shows that by his own definitions Rousseau cannot establish a social consciousness "which is both a moral consciousness and also preserves the stance of natural man" (p. 32). Similarly, Charvet argues, in *Emile* Rousseau abandons the idea—expressed in the *Discourse*—of man's natural self-sufficiency, and he arbitrarily redefines "pity" in such a way as to reconcile what in his original definitions are irreconcilable, namely, *amour de soi* and *amour propre* or, in other words, nature and culture. Finally, in his analysis of the *Social Contract* Charvet exposes the "incoherence" of Rousseau's political solution to the social problem. He closes with a harsh judgment of the "fraudulence" of Rousseau's supposed identity of the individual and the common interest: "The absurdity and incoherence of Rousseau's theory lie precisely in the elaboration of a social ideal founded on the rejection of the right of individuals to live and value each other in their particularity" (p. 146).

Rousseau himself was well aware of his ambiguities and apparent contradictions; he also knew that the whole was often greater than the sum of its parts. Baczko shares this view, and in his study he emphasizes the whole—"the structure of the entire work." Such an approach may cancel out or transcend contradictions; it leads in this case to an interpretation of ambiguities and opacity as logical and historical manifestations or reflections of an age in ferment, a transitional age. Baczko's emphasis on "structure" and "dialectics" and on Rousseau's unconscious, as well as his conscious,

intentions and premises flows from his conceptual and methodological perspective, which is strongly influenced by Hegel, Marx, and Lévi-Strauss. Baczko, following other commentators, includes them among the continuators and clarifiers of Rousseau's thought. Thus, Kant thought through Rousseau's ethical ideas; Hegel worked out Rousseau's dialectic of liberty and history ("We cannot comprehend the *Phenomenology of Mind*," Baczko writes, "without first understanding *Emile* and the *Social Contract*, the preferred reading of the Tübingen seminarian"); Marx clarified Rousseau's notion of alienated and alienating man; Freud thought through Rousseau's emotionally charged expressions of sentiment. Baczko writes that this perspective entails viewing Rousseau not only through his own writings and in the context of his time but also through the lens of the various "rousseauismes" that have drawn some at least of their inspiration from Rousseau's works. (Baczko refers to this as the "second life" of those works.) So viewed, Rousseau's *oeuvre* may be seen as an impressive and unified totality, a structure, in which complex and truly significant themes are treated in more or less greater detail with a prescience and intellectual fecundity that none of Rousseau's contemporaries matched: alienation, the gulf between nature and culture, the inadequacy of a purely rational, analytical study of man, the meaning and role of "solitude," the relations of history, politics, and philosophy, the problematics of democracy and individualism—Rousseau touched upon all of these problems, and more, and if he did not provide acceptable answers to them he clearly asked the pertinent questions. Given such a grand and complex structure—and Rousseau always insisted his thought had to be considered *in toto*—logical fallacies and semantic lapses such as Charvet has analyzed may be dismissed as venial sins.

For Baczko, Rousseau's work stands as a focal cluster or combination of living ideas that reflect not merely Rousseau's unique encounter with his society but also the central concerns of the collective consciousness of the European mind in an age of a prolonged crisis, one which has continued to this day (see especially pp. 391–93). Baczko notes that Rousseau's political thought manifests "a long term pessimism [derived from] the contradictions inherent in political life which tend always . . . to degrade it" (p. 374). This pessimism tends to support the "rousseauisme" of Lévi-Strauss rather than that of Marx. Marx's richly historical concepts postulate a dialectical progress that promises an eventual end to man's social and self-alienation; Lévi-Strauss's nonhistorical and undialectical concepts minimize man's role in history and offer scant hope that, having fallen from

the grace of nature into the trap of culture, man may escape degradation or even destruction. Rousseau, who in Lévi-Strauss's view founded the "human sciences," would no doubt have recognized himself more in the anthropologist than in Marx. It is to Baczko's great credit that his scholarly exposition of Rousseau's thought leads the reader not only to ponder more deeply Rousseau's meaning and intentions but also to re-examine the contributions of the various "rousseauismes," from Kant to Lévi-Strauss, to our understanding of the human condition and its prospects.

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LESTER G. CROCKER. *Diderot's Chaotic Order: Approach to Synthesis*. [Princeton:] Princeton University Press. 1974. Pp. xiv, 83. \$10.00.

In this suggestive essay, Professor Crocker has attempted a synthesis of every aspect of Diderot's thought by categorizing it into the themes of order and disorder. This synthesis is nothing less than a glimpse at Diderot's way of apprehending reality and, of necessity, includes a discussion of Diderot's view of the cosmos, his moral theory, his esthetics, and his politics. It is a hefty assignment for such a short study. Crocker has long been interested in the way the philosophes dealt with the void left in the European consciousness when the Christian world view came under open attack. He observes that "the effects of the dissolution of the Christian synthesis have not yet been absorbed or remedied" (p. x) and is here particularly fascinated by Diderot, as he has been in his other books on the Enlightenment, because Diderot "saw mankind condemned to re-enact perpetually the struggle for order against unconquerable disorder" (p. 115). According to Crocker, Diderot more than the other philosophes helped usher in the awareness that ours is an age of crisis in ethical thought. Be that as it may, what can Crocker's present synthetic study tell us, in a specific and common-sense manner, about Diderot's thought on such a vast array of subjects?

On the question of cosmic order, Crocker concludes that for Diderot the dialectical opposites of order and disorder "are one, and there is no way to extricate the question from the categories of the mind or to separate it from the subjectivity of the perceiving individual" (p. 51). Crocker's portrayal of Diderot's moral theory is similar. There is a constant struggle between the need for social order and the individual wish to be as disorderly as possible. Unlike his version of an authoritarian Rousseau, Crocker sees Diderot as ultimately ambiguous on this question. Again in politics, Crocker's Diderot can only offer some suggestions for

piecemeal reform. He had no totalitarian structures to set up as Crocker's Rousseau did. Only in the realm of esthetics can Crocker see Diderot as decidedly favoring order. Esthetic order "is grasped by a well ordered and orderly mind, which goes beyond the order (or the disorder) of phenomena to the higher level of truth, that is, to a higher or more fundamental order of the universal and permanent to which access is gained through the ideal model" (p. 57).

Except in the area of esthetic principles, then, Diderot remains for Crocker a brilliant but ambivalent thinker. No one can quarrel with the fact that Diderot saw many sides to a multitude of problems and that he eschewed simple answers. But I rather suspect that Crocker has made Diderot a bit more confusing than he really was. The reason, I surmise, is that this confusion is appealing to Crocker, who quotes Sartre with great affection, when the latter maintains that the ordered world hides intolerable disorders (p. 168). Again, this may be so, but students of Diderot and the Enlightenment are better served by the many books and articles that examine this philosophe in less hyperbolic language, and with more modest ambitions.

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EDWARD SHORTER and CHARLES TILLY. *Strikes in France, 1830-1968*. New York: Cambridge University Press. 1974. Pp. xxi, 428. \$27.50.

To develop a general explanation of strike activity in France, the authors statistically examined nearly one hundred thousand strikes. Their analysis of the quantitative data relies upon sociological approaches. This volume shows why such material and method should be part of the effective historian's craft. Shorter's and Tilly's work is of major importance for research on strikes particularly, and the working class generally, in France and elsewhere. Their book is fundamental to the debate over the whys and ways of historical research.

The authors demonstrate that workers belonging to a professional community—for example, the craftsmen of the July Monarchy and the technicians of the Fifth Republic—strike for shop-floor issues. Toilers in the great industrial *bagnes*, located in traditional centers of working-class protest, drawing inspiration from a tradition of conflict, and replacing the magnet of occupational solidarity with the bond of urban association, strike for other reasons. Beginning with the "mobilization of the working class" in 1880-1914, accelerating during the Popular Front and after World War II, these segments of the industrial proletariat

created their own type of organizations. Their large, bureaucratic, centralized trade-unions act "not [to] settle and negotiate" with employers (p. 28), but to pressure the centers of political power. For them the strike is "explicitly political" (p. 235), "an instrument of working-class political action" (p. 343), designed to influence the French political economy. Shorter and Tilly examine other strike theories—strikes are dysfunctional, caused by national character, or acts of marginal workers afflicted by alienation—and find them without an empirical basis. Psychology is put aside. Statistical and sociological analyses demonstrate that strikes are structurally inevitable, conducted by organizations created as a consequence of French society, and manned by those integrated within that society. This work is an example of how an examination of individual events can produce a satisfactory general explanation of the data.

One virtue of the authors' approach is their ability to see beyond the obvious and focus on the critical. They analyze occupations and uncover the consequent organizational results that determine differences in the form and function of strikes. "Industrialization transforms the nature of strike activity by creating a new organizational base" (p. 234). Occupation forms organization, which in turn shapes strike activity. But methodology that brings the authors results in some areas is less productive elsewhere. To study the effect of economic conditions on strike activity, they create an abstract quantitative model—unemployment, cost of living, prices, wages, and production. This neglects the vital question of what workers concretely perceive to be their economic condition. Although the authors mention "popular mentalities," it is missing in their model. It is, however, of crucial historical importance in this area, and for understanding the political consciousness that the authors state is central to working-class action. Their pursuit of the political causes of strikes leads them into a too narrow definition of both economics and politics. Setting up two models of strike motivation, economic and political, is more a reflection of the needs of the measuring social scientist, anxious to isolate the independent variable, than of historical reality. Why the dichotomy and assumption that economic and political variables are separable? By seeking one or the other they miss the point of their connection. Ten million strikers went back to work in 1968 and received higher wages. But did they not strike for political changes? In 1974 the working-class assault on the Elysée was stopped. The winter of 1974-75 saw rolling barrages of coordinated strike activity. It is upon this terrain, for the moment, that the French working class is fighting to capture the heights of political and economic power. Intent on demon-

strating the political purpose of strikes, the authors unnecessarily separate it from the economic component in political action. The politicized French working class does not make such distinctions. It acts in each area for purposes in both.

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JAN KARL TANENBAUM. *General Maurice Sarrail, 1856-1929: The French Army and Left-Wing Politics*. Chapel Hill: University of North Carolina Press. 1974. Pp. xii, 300. \$12.95.

Between 1900 and 1918 General Maurice Sarrail was the ardent spokesman for the liberal, anti-clerical ideals of the French radicals in an officer corps reputedly dominated by conservatives and Catholics. As such he was the darling of the Left, an object of scorn for many of his fellow officers, and the cause of periodic controversy. Professor Jan Karl Tanenbaum, in this book on Sarrail, deals at length with the various phases of his career, focusing primarily on how it was affected by his association with the Left. In the process, he also provides much pertinent information on Sarrail's role in the origins and development of the Allied campaign in the Balkans and on his postwar tenure as high commissioner in Syria and Lebanon.

For all the solid qualities of Tanenbaum's study, in particular his admirably thorough research, he leaves the reader uncertain as to how seriously the vicissitudes of Sarrail's career should be taken, and how significant the controversies he inspired were. Are they to be understood simply as the consequence of the confrontation between an able, but intransigent, self-righteous individual and an unsympathetic professional milieu? Or do they touch upon some of the real, fundamental issues concerning civil-military relations under the Third Republic? The answer may depend on whether one believes that the incompatibility between the opposing ideals animating the army and the republic constituted a serious military or political problem. Sarrail and his political defenders thought they did. They wanted the officer corps to be not only competent and patriotic but also "devoted to the ideals of the Republic." Ultimately, however, most politicians concluded that desirable as this republicanization may have been for domestic political reasons, it could not be accomplished without impairing the effectiveness of the army. There is little reason to think that they were mistaken or that the government would have experienced any fewer problems in World War I if the officer corps and the high command had been more "republican."

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GERALD H. MEAKER. *The Revolutionary Left in Spain, 1914-1923*. Stanford: Stanford University Press. 1974. Pp. x, 562. \$18.95.

This is a fine piece of historical craftsmanship and a major contribution to Spanish historiography. It is an exhaustive study of the impact on Spain's anarchists and Marxists of World War I, the Russian Revolution, the postwar international workers' organizations, and finally—perhaps most decisively—the postwar depression in Spain with the time-honored attempt to reduce labor costs by breaking the unions. Far from unifying the Revolutionary Left, these momentous events deepened the hostilities among its member groups and retarded an already untried movement by denying it any responsible public role. Meaker's viewpoint is distinguishable but controlled: affirming the need for radical change in Spain, he probes to learn why it did not occur in this, the country that responded most enthusiastically to the Russian Revolution. He follows in detail the activities of anarchists, socialists, and communists up to the euphoria of the years 1919-21, when revolution seemed possible, then through 1921-22 as those movements marked time, and down to the tragedy of 1923. Not only does he delineate with fine impartiality the three main currents, but he differentiates the many factions within each. Although he dignifies each faction by describing the ideological basis for its action, he is forced to conclude at times that *personalismo* was the overriding factor.

But this is a long and enormously complex era, a turning point in European civilization no less than for the labor movement; Professor Meaker himself acknowledges the probability that future monographs may modify his classifications of factions undergoing intensive change. One such may be the description of the Escuela Nueva as Fabian, vulnerable if for no other reason than that its members were so determinedly orthodox in their Marxism. Probably he should have treated some sources more gingerly—for example, the memoirs of Pérez Solís. More credence should have been given to the charge of *agent provocateur* (so often leveled against him) to explain his brusque swings from left to right.

These are small caveats on a major analysis of revolutionary politics in Spain. Eschewing shibboleths, avoiding partisanship, Meaker ties "tendencies" to specific groups of militants reacting to specific events, both within Spain and abroad. The development of Marxism and anarchism in Spain is contrasted with that of Western Europe at large; analogy helps to classify and show coherence in factions. One important result, convincing to all who have studied in depth the Partido Socialista OBRERO de España prior to World War I, is that its model was not Guesde's French party but the

German Social Democratic party. Not least of Meaker's achievements is his ability to create a milieu (undoubtedly more rational than the one that actually prevailed) through his skill as a writer, the breadth of his sources, and the extensive, variegated new facts he presents. This is the first comprehensive chronology of the Revolutionary Left for this era. But Meaker's greater achievement has been to ply, adroitly and with somber judgment, the historian's craft: to analyze, synthesize, and refine the material until it achieves an identity of its own.

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R. A. B. MYNORS and D. F. S. THOMSON, translated by. *The Correspondence of Erasmus*. Volume 1, *Letters 1 to 141, 1484 to 1500*. Annotated by WALLACE K. FERGUSON. [Toronto:] University of Toronto Press. 1974. Pp. xxvi, 368. \$25.00.

Since this admirable volume, first fruit of the Toronto Erasmus translation project, will doubtless command a wide audience, it will be useful to record here the very few instances where the original seems incorrectly rendered (references are to P. S. Allen's Latin text): 20.76, 21.42: the *conversus* is more unambiguously called a "lay-brother"; 45.31: for *senatus Christianus*, both common usage and the context here (Erasmus quotes a letter of Filelfo to Charles VII occasioned by a French embassy to Rome) would suggest that this phrase refers to the college of cardinals, not to a "Christian council" of France; 78.8: *nostro secessu* refers more plausibly not to "my" but to "our" withdrawal from Paris, that is, by Dutch monks of the order to which Erasmus and his correspondent belonged; 83.68: the point is not that Cornelis was "less than honest in his dealings with Englishmen" but, more precisely, that he did not speak well of Erasmus in their presence; 131.3: *ut cum intelligere nequeam* is not contrary to fact; it indicates rather, as Allen notes, that Erasmus's Greek (a text of Homer is meant) was at this time not very good; 139.12: *apparet pari arte scriptas videri* means not that the letter in question "appears to have been composed with the same degree of artificiality" but that it "appears to seem" (to his correspondent) to have been so composed; finally, 3.24: where Allen retains in brackets a "not" added by Erasmus's eighteenth-century editor but considers it forced, readers ought to have been alerted in a footnote to the emendation, however plausible it may be.

But these and lesser cavils one might raise are far outweighed by the estimable success of Professors Mynors and Thomson. Their translation is clear and faithful to the text, their style is fresh and vigorous, neither stilted nor burdened with

colloquialisms that will seem hackneyed a few years hence. Particular phrases—even some of Erasmus's Latin puns—are rendered not word for word but with English phrases that often seem to catch the meaning about as neatly as can be done (for example, "a man who lives in state" for *vir magnificus*, "coyness" for *prudens animo*). In sum, it is a pleasure to commend to the general reader a new translation—that by F. M. Nichols did not have the benefit of Allen's critical edition—which may fairly be called worthy of Erasmus's Latin.

It should further be noted that the volume has value also for specialists. Those who have devoted some time to the works of Erasmus's youth can best appreciate how useful it will be to have at hand a translation that clarifies a number of difficult or obscure passages. Professor Ferguson's notes list many classical allusions not recorded by Allen, perhaps, as the editors suggest, because the latter assumed his readers would know their Terence and Plautus. Finally, Professor John H. Munro's appendix, listing the weights and values of some forty-five contemporary coins, will in itself commend the volume to those of us struggling sixteenth-century scholars who cannot tell a genuine Rhenish gulden from a *postulaat*.

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C. H. E. DE WIT. *De nederlandse revolutie van de achttiende eeuw, 1780-1787: Oligarchie en proletariaat* [The Revolution in the Netherlands in the Eighteenth Century, 1780-1787: Oligarchy and Proletariat]. Oirschik: H. J. J. Lindelauf. 1974. Pp. 308. 51.50 gls.

If there woefully have been few studies of the Batavian revolution, the period that went before has received even less attention. Yet this study of the events of 1787, which Dr. De Wit boldly calls a "revolution" although it was directed against the democratic forces, is noteworthy not just because it opens up new territory but also because of the author's freshness of vision and vigor of new interpretation. He frankly puts himself under the aegis of the Palmer-Godechot "Revolution of the West" thesis, and he sees the overthrow of the Patriot movement, which had taken the leadership of the United Netherlands in the wake of the disastrous war with England from 1780 to 1784, as a Dutch version of "pre-revolution"—to use Jean Égret's term. De Wit does not drive the analogy too far, however. The aim of the revolution was not to hamstring the stadholdership—the United Provinces' equivalent of monarchy—but to restore its authority and powers; and the forces upon which it rested were primarily the "aristocracy"—the Dutch "regent" class that had no clear equivalent

in French politics—and the “proletariat” (as De Wit, hearkening back to Roman antiquity rather than to the nineteenth century, calls a congeries of *damnés de la terre*, mostly social detritus, with only the Amsterdam *bijltjes*—shipyard workers—gainfully employed among them). Most important, the triumph of the revolution was not at all the achievement of internal forces acting on their own but came upon the initiative and under the guidance of the English ambassador, Sir James Harris (later raised to the peerage as first earl of Malmesbury for his good work), who not only directed, encouraged, and paid the Orangeists, but also planned and oversaw the riots that became the primary instrument in cowing the Patriots, who feared suffering the fate of the De Wit brothers in 1672. The author notes that the fearful instrument of popular rioting had to be protected by the Prussian invasion forces, which found themselves playing a novel and not altogether pleasant role, and that Frederick William II, despite his concern for his sister, the wife of the stadholder, had to be virtually tricked into the invasion. Not content with this much revision, De Wit goes on to put one of the customary heroes of Dutch historiography, Gijsbert Karel van Hogendorp (noted for his initiative in the restoration of the Orange house in 1813), in the role of villain, Harris’s accomplice in the treacherous, traitorous work of overthrowing the legitimate, established government of the Patriots. (The book is privately published; it is hard to believe that its iconoclasm kept it from finding an established publisher, but one wonders.)

This is obviously an important study. Yet it has some significant gaps and flaws. Key terms are used idiosyncratically, in a loose and arbitrary way; the social underpinnings of political structures and movements are given scant attention; narrative and analysis are not always woven together in a fully satisfactory way. For all that, it is a work that will make historians of the Dutch think over some of their pet assumptions and beliefs, because it does not simply turn the customary picture upside down but shatters it and attempts to reassemble it. It would have been good if the pieces had been put together in a fully persuasive way, but iconoclasm and iconography seldom go well together.

HERBERT H. ROWEN
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H. F. COHEN. *Om de vernieuwing van het socialisme: De politieke oriëntatie van de Nederlandse sociaal-democratie 1919-1930* [Toward the Renewal of Socialism: The Political Orientation of the Dutch Social-Democratic Party 1919-1930]. (Leidse Historische Reeks, number 18.) Leiden: Universitaire Pers. 1974. Pp. x, 279. 42 gls.

The growing volume of monographs on the history of European political parties has been enlarged and enhanced by this compact study of the evolution of the Dutch Socialist party (SDAP) in the decade after World War I. Unfortunately, monographs of this type add little to the general knowledge of the evolution of political parties in Western Europe. On reading this meticulously researched and concisely written book, one cannot help thinking of Carl L. Becker’s famous dictum that we all will “know more and more about less and less.”

Indeed, the author restricts his investigation to a very narrow time frame, and one wonders whether it would not have been more logical to carry it through to 1939. Within this time frame he carefully and objectively examines the gradual transition from a brief revolutionary phase in 1918-19 to the long-range evolutionary development—similar to that of other European socialist parties. Apart from a few references, however, he fails to set this development in relation to the evolution of these other parties.

A comparative overview of this historic process, at least in the decade under review, would have helped the reader to understand the struggle between the reformist and the revolutionary Marxian socialists within the wider context of the genesis of Western socialist parties. The SDAP, after all, was not very different from such fraternal parties as the German SPD, the French SFIO, or the Swedish SAP.

The changes among the leadership of the SDAP symbolize the transformation of the SDAP from orthodox Marxism to pragmatic gradualism. The author offers a great deal of useful and new information on the successive leaders of the party from P. J. Troelstra, the militant activist, to his successors W. H. Vliegen and J. W. Albarda and the trade-union chief, Roel Stenhuis. The latter’s abortive attempt to reorient and revitalize the SDAP largely failed, in Cohen’s opinion, because of the bureaucratic inertia that had overtaken—if not taken over—the party.

After dealing with the left-wing opposition, again a problem common to many European socialist parties in the interbellum period, the author turns to a discussion of the evolution of socialist theory and ideology in the twenties. In this connection he traces the views and influence of the religiously inclined group within the SDAP. In particular, he analyzes the role of the Belgian socialist thinker Henri de Man, who in later years tended to collaboration with fascism. Christian concepts and motivations within the Dutch Socialist party, however, were neither a new nor a unique feature of the SDAP, the party having gone on record even before World War I as being tolerant and pragmatic in religious matters.

In his well-reasoned conclusion the author as-

serts that the political leadership kept the SDAP from a needed conceptual and activist renewal. This, of course, could be said of many of the fraternal parties in the two decades between the world wars. The leadership, however, did not differ much in its disposition from the material and practical concerns of much of the membership of the party and its electorate as a whole. As Cohen rightly points out, it took the crises of the depression and the coming of World War II to shake the SDAP, and many other European socialist parties, into a reformism and revitalization that only in the postwar era made them parties with a national rather than a class appeal. It also led them into positions of power and governmental responsibility that, in the case of the SDAP, had been denied before 1939.

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TAGE KAASTED. *Konseilspraesident C. Th. Zahles dagbøger 1914-1917* [President of the Council C. Th. Zahle's Diary 1914-1917]. (Jysk Selskab for Historie.) Aarhus: Universitetsforlaget. 1974. Pp. xii, 148.

Carl Theodor Zahle (1866-1946) was a Danish lawyer, editor, political party leader, and statesman, whose career spanned about forty-five years of his country's history. A liberal who fought to alleviate the conditions of the tenant farmers and also sponsored much progressive legislation, he served as prime minister on two occasions, 1909-10 and 1913-20. His second term as head of the government coincided not only with the First World War but also with a time of great change as far as Danish domestic policies and practices were concerned.

Some of the best-known members of the second Zahle cabinet, like Ove Rode and Peter Munch, left behind them huge archives, but Zahle's extant writings are limited to a great number of letters and other papers as well as his diary, which has now been made available.

Like most political diaries, the present one throws at least some light on the history of the period. Actually a collection of fragments, it will enlighten the reader about some of the matters then being discussed or acted upon by the Danish cabinet, whose main task during most of that time was to try to steer a middle course between the Allied and the Central powers.

The reader will get to know Zahle better as a person and will get more than an inkling of his relationship with King Christian X and such personages as Edvard Brandes and Erik Scavenius. The inside view provided here of conferences and negotiations, many of them carried out under

great stress, is perhaps of no more than limited interest to readers concerned with the history of Denmark in a general way but will no doubt be warmly welcomed by professional historians, who, incidentally, will be spared the chore of trying to decipher Zahle's handwriting.

ERIK J. FRIIS
Scandinavian-American Bulletin

ERIK BEUKEL. *Socialdemokratiet og stationeringsproblemet 1952-53: En sikkerhedspolitisk beslutning* [The Social Democrats and the Problem of the Stationing of Foreign Troops 1952-53: A Political Decision on Security]. (Odense University Studies in History and Social Sciences, volume 15.) [Odense:] Odense University Press. 1974. Pp. 72. 20 D. kr.

Seven years after World War II ended for Northern Europe, one of the lesser problems that it brought in its aftereffect had to be faced by the Social Democratic party of Denmark. Should it acquiesce in the request made by the Western powers to be allowed to station troops in Denmark so that air bases and other such necessary installations would be better defended against sudden attack, coming, as everyone knew and no one said, from the East? This little volume details how the party decided that it should oppose such occupation.

The author finds little evidence that the Russian occupation of Bornholm in 1945 or the Russian occupation of the Norwegian Finmark in the same year had any influence on the decision. Nor do the party leaders and members seem to have harked back to their experiences with the Allied forces during the perilous times just after the German surrender. Two things seem to have had the really important effects; visits by Social Democrats to the United States and the emergence of a Danish feeling that the country would be better off without any organized force of strangers, foreigners, and outlanders within its boundaries.

Viewing the United States in the fall of 1952 and the spring of 1953, Social Democrats preferred to work with a Democratic regime rather than with the Republican one voted into power in 1952. The United States, involved since 1950 in Korea, had little interest in European adventures. But early in 1953 Eisenhower began the long series of moves that at last led to an end to the Korean War, while the death of Stalin in 1953 opened new vistas for possible action to lessen Russian dominance in central Eastern Europe.

Denmark had been a small country used as a pawn on the chessboard by greater powers. With Stalin dead the Social Democrats saw less chance of Russian aggression; with the United States seeking peace in Korea, it too might not move to create new international tensions. Most of these things

are hinted at, not clearly expressed, yet the author cannot but find that they nourished a Danish idea that perhaps for the next few years Denmark might be better off the fewer possibilities it gave for any incidents of any kind. So the Social Democrats decided on no outside force on Danish soil, and time supported their decision.

FRANCIS J. BOWMAN
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PAUL M. HAYES. *Quisling: The Career and Political Ideas of Vidkun Quisling, 1887-1945*. Bloomington: Indiana University Press. 1972. Pp. 368. \$12.95.

The message of this biography is that Quisling was promising in his early years, dedicated, diligent but somewhat deluded most of his life, usually dull, and finally a failure. Unfortunately, in this case the medium is very much like the message. The book opens promisingly enough with an exploration of Quisling's childhood, youth, and developing character traits. It reports briefly but fairly effectively on his military career and on his diplomatic, administrative, and business activity in Russia after the revolution and throughout the 1920s. In dealing with the political career of Quisling, however, beginning in 1930, the book, like Quisling, begins to go awry. From that point on Hayes concentrates upon politics, increasingly to the exclusion of all else. And that is fatal. Hayes tells us that Quisling was no politician, that his "enigmatic character" can be understood only "in the context of his life." But it is precisely this context that is missing from the bulk of the book. Quisling's Ukrainian-born wife, with whom he lived—and presumably communicated—for twenty-two years, is not even mentioned from 1930 until August 1945 when it is suggested that Quisling, awaiting trial, may have been worried about her. All the rest of his private life, his family, friends, finances, hobbies, tastes in clothing, food, music, art—all the things that give dimension to a man—are missing as the author concentrates in a narrowing fashion upon political events.

We learn, without surprise, that Quisling's efforts to launch a political movement, Nasjonal Samling, were pathetic failures. He and his party were repeatedly rejected by the Norwegian electorate. We also learn that Quisling did, indeed, become a traitor both before and after April 9, 1940. But Hayes is captured by his materials—this is often the case with dissertations—and as the war approaches, the book reads more and more like not only Quisling's but everybody's appointment calendar. The diligent recitation of who went where and who met with whom becomes a maelstrom sucking in ever more people. And Quisling is not in the vortex, neither do we see the devel-

opment through his eyes, in fact he virtually disappears in the froth. This may be the author's artistic way of indicating that Quisling was in over his head. In a biography, however, one should not let the principal get out of focus even if he is peripheral and the events are more interesting.

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ERICH KÄHLER. *The Germans*. Edited by ROBERT and RITA KIMBER. Princeton: Princeton University Press. 1974. Pp. ix, 305. \$13.50.

Nietzsche found it characteristic of the Germans that the question "what is German?" never died out among them. It is equally characteristic that the German question is conventionally posed in terms of "characterology" and "folk psychology." The form of the question is perhaps more revealing than the many possible answers. In his brilliant study, *Society and Democracy in Germany* (1967), Ralf Dahrendorf observed that such approaches were symptoms of the problem of which they purported to be the cure.

Erich Kahler's study, although it claims to be somewhat heretical, is orthodox to the point of trotting out all the faithful old hacks of cultural history. The present work (published posthumously from a set of lectures and based in part on Kahler's giant but unfinished earlier study, *Der deutsche Charakter in der Geschichte Europas* [1937]) postulates that "the history of a people is that people's character—their characteristic being—as revealed in time." Using concepts and images drawn from psychology, literature, myth, and philosophy (inferiority complex, ambivalence, protracted puberty, and a host of hoary dualisms), Kahler evolves the characterological perspective that he finds particularly relevant to German history. He finds that German "being" consists of "becoming"—to have their identity called into constant question, resulting in unmediated swings between extreme impulses. Lacking a stable ethnic, geographic, or political base, German evolution was of the anomalous "centripetal" type, which in contrast to centrifugal, transformative, and divisional types, is in fact no type at all—hence the exaggerated German drive to national identity. (A normal or natural pattern is presupposed but not articulated.)

Characterological history must be genetic in a special, personified sense: the child is father to the man. German "destiny" was set early in the pre-conscious stage by the Germans' ambivalent attitude toward Roman-Christian civic culture and molded by the hollow, atavistic forms of the Holy Roman Empire. To overcome their handicaps, the Germans resorted to either slavish imitation or

imperious bluster. Like their territories, their myth, religion, philosophy, and language lacked objective form, clear boundaries, and a stable sense of values. Kahler uses etymology as explanation and *Wortweisheit* to show this pattern. From the plethora of principalities, Prussia emerged as "power without community"—"a social and political homunculus" and archetype of the total state. Luther likewise figures as the "prototype of the modern German": an unstable composite of conflicting roles and tendencies, who achieved grace only at the cost of partitioning life into an external world of sheer obedience and an internal world of purely spiritual freedom. German unification was forced from above, reinforcing and yet disappointing the desire for inner community. After 1890 the Germans emerged "as the upstarts they were" and tried to find their identity in limitless aggrandizement. "National Socialism was nothing new; it was simply the old carried to extremes." The child is not only father to the man—but mother, sister, brother, and everything else.

Kahler's study is the best of its type; he is certainly more convincing than a Hellpach or a Jaspers. He succeeds in illuminating what he called in his earlier work "the repeated mutual failing of Germany and Europe." But the *déformations professionnelles* only confirm the self-images and tendencies of traditional German historiography: long on dualistic metaphors and pleonasm, short on concrete analysis. We are told that poets and philosophers do not make revolutions and yet we get more on Hans Sachs than on Hitler, a page on St. Simon and next to nothing on German Social Democracy. One cannot avoid observing that Kahler's method makes use of practically every premise of the tradition he sets out to criticize. Although there is a respected place for *Kulturgeschichte* and its variants, it cannot bear the load of the German question as we ask it in the twentieth century.

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Biographisches Wörterbuch zur deutschen Geschichte. Founded by HELLMUTH RÖSSLER and GÜNTHER FRANZ. Second revised edition by KARL BOSL *et al.* Volume 2, I–R. Munich: Francke Verlag, 1974. Pp. x, 1268–2416. 180 fr. S.

The second volume of the Bosl-Franz-Hofmann biographical dictionary for German history follows the format of the first. There are fewer Nazi biographies, and those listed deal with lesser figures. More space is devoted to important dynasties and medieval emperors. The excellent modern German scholarship in this area shines throughout. Maria Theresa is considered an important ruler, but the literature on her domestic reforms has not been

assimilated. Joseph II tends to be glorified for his great power aspirations, and some of his mother's reforms are erroneously attributed to him. Although contemporary American scholarship is often included in the excellent short bibliographies, some of the more standard works tend to be omitted, for example, my own work on Karl Friedrich of Baden's physiocratic reforms. Important political theorists of the earlier period are given their due, especially those who specialized in constitutional law: Limnaeus, J. J. Moser, and Pütter. Kant's revolutionary sympathies are understated, however, while Gottfried von Leibniz is justified by Einstein's theory of relativity. Leopold von Ranke's universalism is sacrificed to a summary of his national histories, and although important figures of the Enlightenment like Hermann Samuel Reimarus are given proper attention, his son Johann Albrecht is more important than he appears here, particularly for his economics. The excellence of recent scholarship on the Catholic Enlightenment in Austria and Bavaria, however, is evident in the entries on Ickstatt, Lori, Martini, and Montgelas. It is interesting to observe that Franz concludes, "Marx's socialism is not actually revolutionary, he rejects anarchism."

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T. C. W. BLANNING. *Reform and Revolution in Mainz, 1743–1803.* (Cambridge Studies in Early Modern History.) New York: Cambridge University Press, 1974. Pp. x, 354. \$21.00.

In the latter half of the eighteenth century, the electorate of Mainz superficially was as ripe for reform or revolution as any European state. But when the electorate confronted its problems, reforms were limited in scope and content, while a local revolutionary tradition proved virtually nonexistent. The story of how and why this happened is the subject of T. C. W. Blanning's fine example of local history.

Blanning's interpretation recalls to mind the verses of the *Fable of the Bees*. "Luxury employed a million of the poor, / And odious pride a million more." For, like Bernard Mandeville, Blanning argues that private vices became public benefits with the result that in Mainz political conservatism left the community continually at odds with the values and attitudes of its policy makers. Thus Blanning portrays a drama of change and continuity as one of irony and paradox. Conspicuous consumption by the imperial nobility promoted the development of a prosperous economy and strengthened ties of loyalty among nobles, peasants, clergy, craftsmen, and merchant guildsmen. Exploiting popular piety, Catholic clergy

sabotaged efforts to create a soundly financed and secular public school system. Mainz businessmen petitioned French Revolutionaries to spare the nobility from expropriation, while a xenophobic citizenry resisted the prospect of self-government and equality before the law. When the electorate disappeared, conservative values and attitudes survived intact.

Blanning's interpretation is designed to fuel the flames of historiographical controversy. He takes sharp issue with Atlanticists and Marxists by playing down the opposition of bourgeoisie and nobility and by dismissing riotous craftsmen and peasants as reactionaries whose demands were "socially discriminatory, economically restrictive, and politically conservative" (p. 266). The company he keeps is generally that of a Holborn, a Krieger, or a Meinecke, who insisted upon German differences. But Blanning is not completely at ease with these either, for he contends that German uniqueness dated from the eighteenth century and originated in the constitutional situation of Mainz as an independent prince bishopric of the empire. Factual and thoroughly researched, Blanning's book is superbly equipped to wage its battles and is highly recommended.

JOHN F. FLYNN
University of the South

RICHARD S. CROMWELL. *David Friedrich Strauss and His Place in Modern Thought*. Foreword by WILHELM PAUCK. Fair Lawn, N.J.: R. E. Burdick. 1974. Pp. 232. \$12.50.

There are few studies in English of David Friedrich Strauss, and Richard Cromwell has attempted to correct the omission with this short, well-written biography. Cromwell states in the opening chapter that his purpose is to characterize Strauss's personality, to trace his development as a thinker, and, according to the title, to establish his place in modern thought. In terms of these objectives, the author has the greatest measure of success with the first—that is, in portraying the man.

Strauss wrote a number of scholarly works, including biographies of Voltaire and Ulrich von Hutten, but it is for his theological writings that he is best known. His first book, *The Life of Jesus*, was published in 1835 and became an immediate sensation because of his claim that the Biblical account of Christ was not historically valid. In retrospect, the year 1835 has been acknowledged by theologians as "the year of the revolution in modern theology" (p. 51). Yet despite his theological radicalism, Strauss was politically conservative. Understandably, it was not long before he broke with the Young Hegelians with whom he had initially been identified.

One major weakness of the book, at least in terms of the author's attempt to identify Strauss's place in modern thought, is the treatment of Left Hegelianism. Cromwell has dealt with Strauss's use of Hegelian philosophy to investigate the life of Jesus, but he has sketched in only the barest outlines of Strauss's connection with and influence on the Young Hegelians. Since Cromwell states at the beginning that Strauss "helped to inspire Left Hegelianism," one would expect him to explain Strauss's role in a movement that affected "the whole course . . . of world history." William Brazill's *The Young Hegelians* (1970) is conspicuously absent from the bibliography.

What mainly concerns the author is Strauss's role in modern Biblical scholarship, and it is here that the chief merit of the book is found. Cromwell investigates the response of such scholars as Albert Schweitzer and Rudolf Bultmann to *The Life of Jesus*. He concludes that not only has Strauss's critique stood the test of time, but also that present-day historical research on the New Testament "stands to a considerable degree on the shoulders of Strauss."

GORDON D. DRUMMOND
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HELGA KROHN. *Die Juden in Hamburg: Die politische, soziale und kulturelle Entwicklung einer jüdischen Grossstadtgemeinde nach der Emanzipation, 1848-1918*. (Hamburger Beiträge zur Geschichte der deutschen Juden, number 4.) Hamburg: Hans Christians Verlag. 1974. Pp. 247. DM 34.

Prior to the First World War, Hamburg contained among its one million people fewer than twenty thousand Jews, roughly three per cent of the Jews in Germany. This solid study of that community, based on manuscript sources in Hamburg and in Jerusalem as well on a full range of statistical and other secondary sources, originally appeared as a doctoral dissertation in 1970.

The Jews of Hamburg, like those elsewhere in Germany, were seeking an identity within which they could be both good Jews and good Germans. Thus their history is not only the story of relations between Jew and gentile, but also a story of tensions within the Jewish community itself. On both these levels the story is, by and large, one of moderate and sober men seeking just compromises. Legal equality came by stages between 1848 and 1860. Jews were active in politics, economic life, and certain professions. Mixed marriages became even more frequent than in Berlin. Political anti-Semitism remained relatively weak.

Intra-Jewish affairs manifested a somewhat typical split in which the orthodox retained their strict synagogue and Talmud-Tora school, while the lib-

erals built a temple and sent their children to interreligious schools. But the leaders were content pragmatically to agree to disagree on certain points and thus retain their congregation (*Gemeinde*) as a single legal entity.

The question arises, then, whether Hamburg could have been a prototype for that "other Germany," relatively tolerant and immune to the appeals of Hitler and his like. By choosing 1918 for the terminal date of her study, Dr. Krohn avoids confronting that question. Indeed one must regret that she chooses to tell only the "middle half" of the story of Hamburg's Jews. For the complexities of the emancipation question prior to 1848, she is content to refer one to her *Magister* thesis (1967) on that subject. It is difficult to avoid the feeling that though the 1848-1918 limitation may have been defensible for a doctoral dissertation, it severely weakens the book under review.

Still, what Krohn selected to do, she did very well. By analyzing a significant segment of German Jewry in a dispassionate and scholarly manner, she has provided another useful addition to the growing literature on the problems of minority groups in modern society.

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FRITZ FISCHER. *World Power or Decline: The Controversy over Germany's Aims in the First World War*. Translated by LANCELOT L. FARRAR et al. New York: W. W. Norton and Company, 1974. Pp. xxiii, 131. Cloth \$6.95, paper \$1.95.

Even historians outside the German field must by now have heard of the heated controversy among German historians in the 1960s caused by the works of the Hamburg historian Fritz Fischer. His two main books—*Griff nach der Weltmacht* (1961; English ed., *Germany's Aims in the First World War* [1967]), and *Krieg der Illusionen* (1969; English ed., *War of Illusions* [1973])—dealt with Germany's major responsibility for, and far-reaching aims in, World War I. To scholars abroad they did not present anything startlingly new, except perhaps a revised image of Germany's chancellor Bethmann Hollweg. But in Germany they touched a raw nerve. Much of the agitation was due to the obvious parallels between Germany's aims in both world wars, a fact which contradicted the assertion by many of Fischer's older colleagues that nazism was a mere "accident" or "detour" on the long road of German history.

The small volume reviewed here is an offshoot of Fischer's major works. It was first published in 1965 as *Weltmacht oder Niedergang: Deutschland im ersten Weltkrieg*. Because it was written at the height of the "Fischer controversy," it is under-

standably partisan. Besides stating Fischer's main theses, the book presents his rejoinders to his most persistent critics, Gerhard Ritter and Egmont Zechlin. Its value is enhanced by a brief introduction, in which Fischer reviews the controversy caused by his writings. The translation is smooth and, on the whole, faithful, except for some of Fischer's criticisms of Ritter, which have been toned down considerably. These departures from the original make one wonder if the translation is perhaps based on the reputedly "milder" second German edition of 1968. If this is the case, it should have been pointed out. As things stand, the second edition is not even mentioned.

The Fischer controversy, meanwhile, has itself become history, as some of his most ardent opponents have either died (Ritter) or virtually come around to Fischer's views (Zechlin). At the same time, a generation of younger German historians, many of them Fischer's students, have been encouraged to approach their country's past with open and critical minds, producing scholarly works of considerable originality and merit. This "historiographical revolution" is perhaps the most positive long-range effect of Fischer's courageous stand on controversial issues.

HANS W. GATZKE
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DANKWART GURATZSCH. *Macht durch Organisation: Die Grundlegung des Hugenberg'schen Presseimperiums*. (Studien zur modernen Geschichte, number 7.) [Gütersloh:] Bertelsmann Universitätsverlag, 1974. Pp. 486. DM 42.

Of all the gravediggers of the Weimar Republic who helped prepare the way for Hitler, none played a more important role than Alfred Hugenberg. Though never a Nazi, he was ready to use fascism as a weapon against democracy and radicalism. There were others who adopted the same strategy, confident that they could manipulate the National Socialist movement to serve their own purposes. But Hugenberg's position was particularly significant, not only because he was leader of the Nationalist party, but also because he exercised far-reaching control over the mass media of Germany. He discovered after it was too late that he could not control the whirlwind. The man who had been so influential for more than thirty years was forced into retirement and obscurity within six months after the establishment of the Third Reich. In the end the gray eminence proved to be only a paper tiger.

In his scholarly and judicious monograph, Dankwart Guratzsch examines the early career of Hugenberg up to the end of the First World War. He sees in him a "technocrat of power," a different

kind of conservative whose influence derived not from noble birth, or party leadership, or vast wealth, but from close ties to the bureaucratic apparatus of state and economy. On the basis of voluminous research, the author describes the successive stages in the rise of his protagonist: government official in the eastern borderlands of Prussia, determined to perpetuate German domination over the Poles; chairman of the board of directors of the firm of Krupp; champion of expansionist war aims after 1914; and most important, founder of a press empire that was not to reach its full dimensions until the Weimar Republic. Throughout this steady ascent to power Hugenberg displayed a remarkable talent for organization combined with a passion for secrecy. To Guratzsch, he is the embodiment of a new type in rightist politics, applying the techniques of managerial efficiency to the shaping of public policy. Too narrow in outlook to become a popular leader, he could only engage in schemes and intrigues designed to maintain an authoritarian form of government in Central Europe.

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INGRID SCHULZE-BIDLINGMAIER, editor. *Die Kabinette Wirth I und II: 10. Mai 1921 bis 26. Oktober 1921; 26. Oktober 1921 bis 22. November 1922*. Volume 1, *Mai 1921 bis März 1922; Dokumente Nr. 1 bis 236*; volume 2, *April 1922 bis November 1922; Dokumente Nr. 237 bis 409*. (Akten der Reichskanzlei Weimarer Republik.) Boppard am Rhein: Harald Boldt Verlag. 1973. Pp. lxxxvi, 658; v, 659–1231.

GÜNTER ABRAMOWSKI, editor. *Die Kabinette Marx I und II: 30. November 1923 bis 3. Juni 1924; 3. Juni 1924 bis 15. Januar 1925*. Volume 1, *November 1923 bis Juni 1924; Dokumente Nr. 1 bis 213*; volume 2, *Juni 1924 bis Januar 1925; Dokumente Nr. 214 bis 388; Anhang Nr. 1 bis 11*. (Akten der Reichskanzlei Weimarer Republik.) Boppard am Rhein: Harald Boldt Verlag. 1973. Pp. lxiv, 676; v, 677–1406. DM 160 the set.

With the appearance of these volumes, the project to publish documents from the Reich chancellery for the period of the Weimar Republic, which produced its initial volume in 1968, nears the half-way point. Nine of the twenty-one cabinets of the Republic have now been covered (for reviews of the previously published volumes, see *AHR*, 75 [1970]: 1150; 78 [1973]: 1489–91).

Like the earlier volumes, these consist primarily of documents from the highest levels of the chancellery's operations: minutes of full cabinet meetings and conferences of smaller groups of ministers with subcabinet officials, representatives of the federal states, and spokesmen of various insti-

tutions and interest groups; memorandums by and for the chancellors and their staffs; legislative drafts; and important correspondence of the chancellery. These official documents have been supplemented by excerpts from, or references to, unpublished papers, as well as published diaries and memoirs, of significant contemporaries.

The documents are arranged in chronological fashion throughout the term of each cabinet. This conveys to the reader a sense of the interplay of the array of disparate problems and events that, at any given time, simultaneously confronted the chancellors and their ministers, something easily overlooked in research on an isolated topic. In the absence of any topical grouping of documents, a researcher who seeks to pursue a single theme or several limited themes will find that task facilitated by the exhaustive indexes of names and topics provided for each cabinet. Anyone wishing to pursue a topic in depth must, however, have recourse to additional sources. The high-level records that make up the bulk of these volumes usually indicate only what matters were dealt with and what decisions were reached, often revealing little about how the decision-making process took place or about the thinking and motives of the participants. In addition it is important to recognize that the documents printed in these volumes represent merely a small segment skimmed from the top of one of the many surviving archival collections of government documents from the Weimar period. The cabinet minutes, which make up the bulk of these volumes, constitute, by way of illustration, only 129 files of a total of approximately 2,780 in the full collection of chancellery documents, which is housed in the West German Bundesarchiv in Koblenz.

Since the chancellery collection has been open to researchers for more than a decade, the volumes under review here contain no startling disclosures. Indeed, their most surprising feature is the number of important topics on which the chancellery files, according to the volume editors, contain little or no information. In the case of the second cabinet of Josef Wirth, for example, the major foreign policy development—the negotiation and signing of the Rapallo Treaty with Bolshevik Russia in April 1922—left only the most superficial of traces in the files of the chancellery. This and other similar gaps could have been remedied by recourse to secondary literature, which in the case of the Wirth cabinets includes not only numerous studies of Rapallo but also a detailed monograph on all the major policies and actions of the government (Ernst Laubach, *Die Politik der Kabinette Wirth 1921/22* [1968]; cf. *AHR*, 74 [1969]: 1316). Secondary works have, however, been rigidly excluded from consideration on the grounds that to include

references to such studies would involve the project in historiographical controversies and in the interpretation of documents. According to the statement of editorial policy by Karl Dietrich Erdmann—who, with Hans Booms, has supervised the preparation of these volumes—the task of interpretation should be left to the individual researcher. Yet despite this stated policy, the introductions by the volume editors, which provide capsule chronicles of the history of each cabinet and a summary of the major issues faced, occasionally contain highly interpretive passages. In the introduction to the volume on the Wirth cabinets, for example, the editor writes, “Wirth not only spoke of fulfillment but also employed the finances of the Reich with idealism and vigor, within the limits of what was possible in the parliamentary system, in order to pursue a policy of fulfillment. Approximately a third of the documents presented in this edition attest to this” (vol. 1, p. lxxi). Those who are familiar with the historiography of the Weimar Republic are well aware how problematical such sweeping assessments are, especially when based upon incomplete evidence, as is admittedly the case here. Moreover, if the volume editors are permitted such editorializing, one is led to wonder why all other interpretive viewpoints should be excluded, along with the often informative additional evidence presented in support thereof.

In technical respects these volumes are, on the whole, models of documentary editing and are handsomely printed in an easy-to-read format. Still, there are a few deficiencies. For example, the foreword by Erdmann, containing the statement of editorial policy and providing valuable information about the history of the chancellery documents and the structure and procedures of the republican chancellery, is inexplicably omitted from the volumes on the Marx cabinet. Missing from both sets under review here is a listing of the previously published volumes in the collection. A researcher who comes across an isolated volume is thus left uninformed about the others already in existence. These technical faults, however, are minor. All scholars interested in the Weimar Republic owe a debt of gratitude to the editors and to the sponsoring institutions, the Bavarian Academy of Sciences and the Bundesarchiv, for what will quickly become a standard and indispensable tool for research on that period of German history.

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Akten zur deutschen auswärtigen Politik, 1918–1945, aus dem Archiv des Auswärtigen Amts. Series B: 1925–1933. Volume 4, 1. Januar bis 16. März 1927; volume 5, 17.

März bis 30. Juni 1927; volume 6, 1. Juli bis 30. September 1927. Göttingen: Vandenhoeck & Ruprecht. 1970; 1972; 1974. Pp. 1, 656; 1, 670; xlix, 567.

The significance of Series B for serious students of German foreign policy in the Weimar era has been stated by Thomas T. Helde in his reviews of volumes 1–3 (see *AHR*, 73 [1967–68]: 523; 74 [1968–69]: 647; 75 [1969–70]: 530–31, 875) so eloquently that it hardly needs underscoring. The same editorial excellence that he observed characterizes also volumes 4–6. The historian dealing with the 1920s will have to supplement them, however, by frequent references to the *Akten der Reichskanzlei* for those years, which are published gradually, and to the microfilms of the Stresemann Papers. Unfortunately, publication projects of this magnitude and complexity progress but slowly.

Volumes 4, 5, and 6, for which Hans Rothfels, Maurice Baumont, Ronald Wheatley, and Hans W. Gatzke serve as *Hauptherausgeber*, represent a major change from its predecessors. The first three had been arranged broadly by subject. The editors decided that henceforth they will print all documents in their chronological sequence. This step has much to commend itself, especially since about thirty-five pages of an annotated subject index in front of each volume make it easy for the reader to follow the development of major topics and areas during the period covered.

The year 1927 was not a particularly happy time for promoting a better understanding between the European powers. Locarno and Thoiry had raised unduly high expectations in the German public for the quick correction of many controversial clauses of the Versailles Treaty. Now people began to realize that Stresemann's French and English colleagues could not deliver these improvements with the desired speed. The position of his opponents at home had been strengthened in late January 1927, when four German Nationalists under Oskar Hergt's leadership joined the fourth cabinet of Chancellor Wilhelm Marx. They were among the politicians who blamed Stresemann for *Erfolgsarmut* when he returned without big successes from his quarterly “summit” conferences with his Locarno partners in Geneva. Several documents show that Stresemann himself was irritated by Briand's evasiveness when the reduction of the occupation forces in the Rhineland was under discussion. But Stresemann was gratified when Sir Austen Chamberlain at the June session of the League of Nations Council praised the good relations between Berlin and Moscow and appealed to his German colleague to serve as an intermediary in a serious Russo-Polish conflict. At the September meeting, Chamberlain and Stresemann saw

eye to eye also concerning Polish wishes for some kind of an "Ost-Locarno."

As one reads through the more than seven hundred documents printed in these three volumes, he must be impressed by Stresemann's astute dealings with such different personalities as Briand, Chamberlain, and Chicherin. It also becomes quickly evident that he was supported by a highly competent team of diplomats, among them the state secretary Carl von Schubert, the ambassadors von Hoesch (Paris), von Maltzan (Washington), and the irascible but brilliant Count von Brockdorff-Rantzau (Moscow), and the legal adviser Friedrich Gaus.

An appendix to volume 4 offers a bonus to students of Stresemann's foreign policy: the full text of an address he delivered at a conference of the Reichszentrale für Heimatdienst on January 28, 1927, about Germany's place among the nations of the world. This is a fine example of his forceful oratory; it supplements and enriches this collection of diplomatic documents. It is a cause for regret, however, that the sixth volume does not include Stresemann's moving address before the League Assembly on September 9, 1927 (see Stresemann, *Vermächtnis* [1933], 3: 180-87). I was present when it was delivered and can testify to the deep impression it created on his audience. As a masterpiece of eloquence it was surpassed only by Aristide Briand's fiery appeal for peace from the same forum the next day.

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DONALD M. MCKALE. *The Nazi Party Courts: Hitler's Management of Conflict in His Movement, 1921-1945*. Lawrence: University Press of Kansas. 1974. Pp. xvi, 252. \$10.00.

This well-researched study is the first to describe the activity of the *Parteigerichte*, the Nazi party's official mechanism for controlling internal discord. Created in July 1921, the *Uschlas* (*Untersuchungs- und Schlichtungsausschüsse*), or investigation and conciliation committees, were reorganized in 1926 as part of Hitler's effort to create a tightly knit, centralized, bureaucratic party. Established at every administrative level of the movement, the network of *Uschlas* had its powers defined before Hitler became chancellor and underwent little change in subsequent years. From 1927 until the fall of the Third Reich, Walter Buch, a former battalion commander and a fanatical racist, acted as the party's chief justice, although from 1941 his personal power was systematically undermined and reduced to almost nothing by his son-in-law and bitter enemy, Martin Bormann.

The *Uschlas* had several functions. In addition to mediating quarrels within the party and enforcing

discipline, the committees checked all membership applications to prevent infiltration by such racially or politically undesirable elements as Jews, Freemasons, or Communists—a task that assumed mammoth proportions in 1932-33 with the mass influx of membership petitions. While assuring loyalty to Hitler, the courts also provided him with a tool he could dominate and one that would take responsibility for any unpopular or potentially divisive decisions. Rather than take sides between powerful rivals the Führer could remain aloof. Thus, in the case of the Berlin struggle between Goebbels and the Strasser brothers, Hitler avoided action by placing matters in the hands of Buch until he had consolidated his power against the Strassers in the north.

Behind their façade of legality, the courts remained strictly subordinate to Hitler and the party leadership. They were prevented from taking action against party dignitaries like Streicher, Ley, Frank, or Amaan, even when there was incontrovertible evidence of embezzlement or other misuses of power. For the average German, however, the courts played an important role in the Nazi regime's arsenal of intimidation. Increasing numbers of party members were hauled before them, especially as the war situation and conditions within the Reich deteriorated. If their power of expulsion from the party had been a limited punishment before 1933, subsequently it could easily mean loss of employment, conscription for the front, or arrest by the police and confinement in a concentration camp. The *Parteigerichte* acted as a powerful instrument in destroying opposition to the regime both before and during the war.

McKale's careful and important book rests on a variety of source materials and offers valuable insights into the inner workings of nazism. Greater use could have been made of the records to show in detail the incidence of certain types of offenses, the social backgrounds of those tried, and the punishments meted out to them. This book tends to concentrate on the more notorious cases such as the acquittals of confessed murderers after the *Reichskristallnacht* in 1938. I hope that later studies will elaborate further on the Nazi machinery of judicial control and terror from the perspective of its victims.

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ILSE MAURER. *Reichsfinanzen und Grosse Koalition: Zur Geschichte des Reichskabinetts Müller (1928-1930)*. (Moderne Geschichte und Politik, number 1.) Bern: Herbert Lang. 1973. Pp. 269. 37 fr. S.

The thrust of this study, which illuminates the collapse of the last democratically established cabinet of Germany in March 1930, relies heavily on

the standard political works of Bracher, Conze, Morsey, and others. Maurer's contribution lies in her meticulous research and explanation of the crucial role that financial policies played in the work of the Weimar cabinets from 1924 to 1930. Her study demonstrates how the cumulative dilemmas of these cabinets in facing the growing financial problems of those years became the lever by which the Center and right-wing parties forced the Social Democrats out of power in order to install Brüning's minority cabinet backed by Hindenburg's emergency powers.

The author systematically interrelates international financial factors with internal German fiscal problems. Economically restored between 1924 and 1927, the republic thereafter waged a losing campaign to consolidate internal fiscal reform while there was still "prosperity" in the Western world. Indeed, Germany was already in a stubborn recession by the autumn of 1928. When the Müller cabinet was formed as a "Grosse Koalition," its basic element of cohesion lay in pursuing foreign economic and political policies. From the outset the coalition was not agreed on policies of financial reform. Its leaders hoped against hope that easier reparations payments to be negotiated under the Young Plan of 1929 would cover the emerging internal deficits so that reform measures could be negotiated among the parties more slowly and steadily. Of course, all this backfired with the antireparations propaganda of 1929-30, the coming of the depression, and the accompanying devious maneuvers of the Reichsbank president, Schacht. The rapidly deteriorating financial situation of the republic was the crucial area of jockeying for power that produced Chancellor Brüning, an apparently lackluster economic administrator who nonetheless worked vigorously to convert the republic into a constitutional monarchy.

Maurer's detailed analysis of these financial and political affairs does not make for inspired prose. Yet, a careful reading of the book alerts us to the escalated financial problems and political byplay that no doubt currently preoccupy Schmidt, Giscard d'Estaing, and Kissinger.

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GÜNTER PLUM. *Gesellschaftsstruktur und politisches Bewusstsein in einer katholischen Region, 1928-1933: Untersuchung am Beispiel des Regierungsbezirks Aachen.* (Studien zur Zeitgeschichte.) Stuttgart: Deutsche Verlags-Anstalt, 1972. Pp. 318. DM 26.

Günter Plum's study of political attitudes in the region of Aachen during the final years of the

Weimar Republic is full of detailed analysis and perceptive comments. The first half of the book traces the rapid social and economic changes that swept over this border area following Germany's defeat in 1918—the occupation, inflation, and subsequent depression years. Dislocation, unemployment, trade barriers, and lack of investment played havoc with both the industrial and agricultural sectors. Rhenish combated with national feeling, democratic sympathies competed with the desire to return to the familiar authoritarianism of the past, and economic conflicts were reflected in the political struggles of Left and Right. Plum's analysis of the political consciousness of this region, and especially of the impact of the Catholic Church and its organizations, is derived from a meticulously close study of local sources, tracing the waning but still powerful hold the Church retained.

The second part describes more generally the relations between the Catholic Church and the Weimar Republic and demonstrates the readiness of the Catholic leaders to move to the right in opposition to the splintering effects of party politics, the growth of pluralistic social patterns, and the scare of Bolshevism. Plum rightly points out that the Catholic rhetoric, which branded all Socialists and Communists as "godless," attacked all liberalizing tendencies as "immoral," and tried to prove its national loyalty by wholehearted opposition to the Treaty of Versailles, prepared the ground for the capitulation of the bishops in early 1933.

The illusions about National Socialism shared by so many Catholics, and the disillusionment with the Center party that was abandoned so recklessly, are here excellently depicted. On the other hand, Plum seeks to show that the basic discontents of the Aachen region were not removed by Hitler's seizure of power. In fact, as Bernhard Vollmer has shown in an earlier study published by the same Institut für Zeitgeschichte, opposition to the Nazi state grew in the Aachen region steadily from 1933 onward, and the Catholic Church became the focus point of disaffection. Plum's analysis of the pre-Nazi years shows that political consciousness was as much influenced by economic circumstance as by ideological indoctrination. In contrast to more favored areas of the Reich, the Aachen region suffered from neglect. Catholic opposition both to the Republic and to the Third Reich can be ascribed to the Church's acting as a vehicle for this continuing discontent. But the teaching of the Church in favor of obedience to the state kept this opposition from becoming a more powerful force and was duly exploited by the Nazis for their own ends.

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NORMAN RICH. *Hitler's War Aims*. Volume 1, *Ideology, the Nazi State, and the Course of Expansion*; volume 2, *The Establishment of the New Order*. New York: W. W. Norton and Company. 1973; 1974. Pp. xliii, 352; xv, 548. \$10.00; \$14.95.

These two handsomely produced volumes succeed notably in the effort "to describe the interaction of Hitler's words and deeds and record the development of his war aims on the basis of the policies he actually pursued in Germany and the German occupied countries." The initial nine chapters give a skillful, pointed, but perhaps meager, background sketch of Hitler's outlook, or ideology, and instruments of power and conquest. The author does not describe the dominating association of SS and police with concentration camps and other measures of terror, passionately driven by Hitler's dogmas, which S. Aronson impressively showed characteristic of the German dictatorship from the beginning. Twelve chapters survey the largely triumphant course from the initial speed-up of the renewal of German military force, accompanied by public professions of peaceful intentions and claims for "equality of rights" and national self-determination, through the attack to destroy the Soviet Union and the declaration of war on the United States. For a compact work of some 170 pages, it is hard to surpass this carefully documented account that rests firmly on contemporary materials available through Nuremberg tribunals, other war crimes trials, and published diplomatic documents. Rich also used extensive scholarship effectively and details evidence in some twenty-nine of forty-seven pages of notes, inconveniently located at the end of the volume but helpfully numbered to the page of the text.

With many nuances and indications of points of inconclusive evidence, he convincingly demonstrates that conquest of land from Russia, Lebensraum, dominated purposes: "Hitler did indeed have a definite expansionist program which derived from his ideological conceptions." His sense of mission and passion for speedy results dominated timing.

The second, longer volume provides an authoritative, comprehensive survey of Nazi occupation policies and should be a standard reference work. Through records of German administrations, many microfilms of captured German documents, and published and unpublished materials, as well as scholarship (all cited in fifty-five pages of notes), Rich examines policies for conquered peoples and plans for their future, rightly seeking "perhaps the most authoritative evidence about the ultimate nature of Hitler's war aims." The results delineate "Hitler's insistence on implementing his ideological program while the war was still in progress and before a final German victory had been achieved."

Mentioning Hitler's governmental attacks on Jews in Germany from early 1933, Rich briefly recalls the victimization of the dogmatically defined enemy of the "master race." Then eleven chapters survey the treatment of each conquered country, following almost exactly the order in which they fell to German power. He relates special circumstances, balanced accounts of persons, and the many overlapping and competing German administrations that controlled, confiscated, exploited, and uprooted peoples, and in general "Germanized," while incorporating or planning to incorporate the chosen into the Greater German Empire.

References to activities of private business groups in occupied countries pique curiosity for searching inquiry. Presumably, concentration on Hitler's war aims turns study away from developed assessment of changes in German policies and practices as failure in Russia opened up possibilities of defeat or as exploitation to overcome German deficiencies in the long war became ever more ruthless. The careful surveys of attacks on Jews in each country add information to scholarship used—most frequently, works by Raul Hilberg and Gerald Reitlinger.

Rich convincingly demonstrates that only insistence on bringing the apocalypse of *Mein Kampf* to conquered Europe during the war explains the vast destruction of Jews and Slavs by governmental policy, which continued to the eve of defeat. It also drained German strength from the war of conquest. Even without the useful accounts of Hitler's plans and the author's speculations, no attentive reader should fail to note what devastation the defeat of Germany saved the world from.

The text displays, as the title does not, that many Germans shared Hitler's war aims, but the author did not seek to investigate those relations. Delineation of the size, operations, changing conditions, and developments of the administration in each country also remains to be worked out, as comparison simply of the Ukraine, Czechoslovakia, and France shows. Recognizing many ramifications of his subject, Rich sticks to his purposes, as is his right, especially when accomplishing so much, without asking how Hitler's war aims related to the goals of German political and military leaders in the War of 1914-18—similarities stand out—or to ambitions of Germans in the twentieth century, or other problems arising from broader perspectives. He limits himself to observations on the cult of racial nationalism.

Each volume has a splendidly extensive bibliography, eight convenient and informative maps, and, respectively, twenty-four and sixteen pages of photographs, well chosen from different collections, often with revealing glimpses of the perpetrators. Each volume has an appendix of brief

"biographical sketches," the second of which provides a convenient list of main participants in the German domination of Europe.

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ADOLF BEER. *Die Finanzen Österreichs im XIX. Jahrhundert, nach archivalischen Quellen*. Reprint; Vienna: H. Geyer. 1973. Pp. xi, 458. Sch. 493.

One of the more prolific writers of the nineteenth century on Habsburg economic history, Adolf Beer was a teacher and politician, whose moderately liberal learnings are evident in this volume. His style is based to a large extent on a paraphrasing of the primary sources that he selected judiciously in the archives. While not exciting history, Beer's works have the merit of accuracy. They cannot be omitted from the bibliography of any serious student concerned with Habsburg industry and commerce and the government's finances. The publisher of this reprint and of a number of other classics in Austrian history, H. Geyer in Vienna, has also brought out Beer's monographs on public finance under Maria Theresa and on Austrian commercial policy in the nineteenth century.

The present volume, published originally in 1877, deals with public finance of the Austrian government in the nineteenth century. Its first part concerns the history and the collapse of the government's financial system, especially the so-called state bankruptcy of 1811. The author is sharply critical of the finance ministers of the time, Counts O'Donnell and Wallis, as well as of Emperor Francis. Among Beer's heroes are Count Stadion, Wallis's successor as finance minister and a person who brought some order into this government department. Other heroes include a subsequent finance minister, Freiherr von Kübeck, whose famous diaries are one of the best sources for the political history of the so-called Vormärz era, the period preceding the revolution of 1848, and Freiherr von Bruck, the minister of economic affairs in the 1850s, who was a major stimulator of the monarchy's drive toward industrialization.

One surprising hero also slips in. Prince Metternich, for whose opinions on internal finance Beer has few plaudits, comes off well indeed in a chapter on the tariff system in which the minister proves to be an early advocate of Austria's joining in some sort of tariff union with Germany.

Beer's primary criticisms throughout concern the chaotic organization of the central government and the exorbitant demands of the military establishment, both subjects having a somewhat present-day ring to them.

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WILLIAM J. MCGRATH. *Dionysian Art and Populist Politics in Austria*. New Haven: Yale University Press. 1974. Pp. 269. \$12.50.

This is an analysis of a remarkable group of Viennese intellectuals whose careers spanned the last generation of the Austrian Empire. Emphasis is on the way in which the individuals of the group expressed in their careers, in a wide spectrum of political, artistic, and professional activity, the particular philosophy of life that the group as a whole had created. The principal members were Victor Adler, Engelbert Pernerstorfer, Gustav Mahler, Siegfried Lepiner, Heinrich Friedjung, and Max von Gruber. At one time or another Georg von Schönerer, Richard von Kralik, and other prominent politicians and esthetes of *fin-de-siècle* Vienna were in or around the group and influenced by it.

Professor McGrath traces the origins of this group to the famous Schottengymnasium in Vienna in the late 1860s, when the Benedictine teachers began to shape their students' life-long "search for the poet-priest." Other influences on the group were the prevailing contemporary adolescent mood of rebellion against classical liberalism and the Habsburg tradition, despair at Bismarck's exclusion of Austria from Germany, the radical nationalist cult in Austria of bizarre Prussophilia, and the natural sympathy of idealists with the victims of advancing industrialism. But the most important intellectual inspiration came from the writings—and the vulgar popular vogue of the day—of Schopenhauer, Nietzsche, and above all, Richard Wagner. "Wagnerism," as McGrath calls this vague and murky amalgam of intellect and emotion that emanated from Bayreuth, emphasized the unity of all forms of culture and what it thought of as its "Dionysian abandon" over the "Apollonian restraint" of liberalism and scientific rationalism. It rooted the true bonds of brotherhood and social order in emotional feeling and indulged in fantasies of "spiritual experience as truth," as opposed to the experience of reality. Art, for the Wagnerians, would provide the inspiration for a complete regeneration of society. These Wagnerians thought of their activities as a "higher form" of the disciplines to which they were attached, a form mysterious and difficult to grasp, to be experienced rather than understood—hence their self-conscious notion of a "metapolitics" and a "metamusic" along with metaphysics. The author shows how activists like Adler used theatrical symbolism to activate mass feeling; how Pernerstorfer aimed at a combination of art, politics, and culture; and how the esthete Mahler tried to use artistic symbolism to create a metaphysical social community that transcended reality. Social regeneration through the mystic

community of the Dionysian theater was the theme that united the politics, scholarship, literature, and music of these members of the so-called Pernerstorfer circle. McGrath argues convincingly that long after the circle broke up, its members remained united by the invisible bond of the Wagnerian outlook of their youth.

The attractive originality and value of this work lies in its exploration of a wide range of political, cultural, and social phenomena, in its breaking down the academic barriers between such disciplines as music and politics, and in its basis in a deep knowledge of the political, economic, and esthetic literature of *fin-de-siècle* Vienna. It is as valuable for the questions it raises as for those to which it offers answers.

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DIETHILD HARRINGTON-MÜLLER. *Der Fortschrittsklub im Abgeordnetenhaus des Österreichischen Reichsrats, 1873-1910*. (Studien zur Geschichte der österreichisch-ungarischen Monarchie, number 11.) Vienna: Hermann Böhlau Nachf. 1972. Pp. 195. DM 40.

The Progressives—or Progressivists, as they were sometimes called—formed a part of the liberal Left in the lower house of the Austrian parliament. They were never a party in any strict sense of the word: there were no whips or caucuses, but they formed a “club” that was not without influence in the penultimate critical period of the Austro-Hungarian monarchy, when the German and centralist elements were increasingly on the defensive against the centrifugal nationalist elements in that multination state, and often found little sympathy for their position from the emperor. Dr. Harrington-Müller’s book is largely a revision of her doctoral dissertation, undertaken in recognition of the importance of the study of Austrian parliamentary activities of a period that has left few official records. She has had to rely extensively on newspaper accounts, especially the *Neue Freie Presse* and the *Deutsche Zeitung*, which gave them extensive coverage, and, of course, the records of the debates of the lower house of the Austrian Reichsrat. Inevitably the reader will be tempted to comparisons with party life in eighteenth-century England—did Namier’s own background in the lands of the monarchy inspire his studies?—and with the challenges to European liberalism in the other nation-states and empires of Europe at the time. Harrington-Müller recognizes two important periods for the Austrian Progressives: the first immediately after the first direct elections to the Reichsrat, in 1873, and the second after 1897, when the enlargement of the franchise had separated the Ger-

mans—now clearly a minority in the Austrian Empire—more sharply into a conservative Right, a liberal Left, and even a small socialist contingent. The earlier period made the Progressives part of a more general “liberal wave,” the second left them the sole remnant of German political liberalism at a time when nationalistic concerns had become the dominant issue. They were a group of men of deep convictions and differing opinions, some Jewish, some almost anti-Semitic, many from parts of the monarchy in which Polish or Czech concerns had become predominant. It was not in their power or interest to form a structured party. Dr. Harrington-Müller serves them well, and helps the reader greatly by adding short biographies of the most important members of the “club” and by an excellent bibliography.

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M. A. POLTAVSKII. *Avstriiskii narod i anshlius, 1938 g.* [The Austrian People and the Anschluss, 1938]. (Akademiiia Nauk SSSR, Institut Vseobshchei Istorii.) Moscow: Izdatel'stvo “Nauka.” 1971. Pp. 200.

GERHARD BOTZ. *Die Eingliederung Österreichs in das Deutsche Reich: Planung und Verwirklichung des politisch-administrativen Anschlusses (1938-1940)*. (Schriftenreihe des Ludwig-Boltzmann-Instituts für Geschichte der Arbeiterbewegung, 1.) [Vienna:] Europaverlag. 1972. Pp. 192.

Recent works on the *Anschluss* by Jürgen Gehl, Ludwig Jedlicka, Ulrich Eichstädt, and others have raised controversial or unanswered questions. Was the Nazi stroke of March 1938 carefully premeditated or an improvised response to last-minute circumstances? Were the Austrian masses favorable toward *Anschluss*, hostile, or apathetic? Did Austria need integration with some larger unit to survive economically? How did the process of *Gleichschaltung* operate to strip Austria of sovereignty?

Writing in a vein common to Soviet historiography, M. A. Poltavskii sees the *Anschluss* as fulfilling imperialist goals espoused by Reich- and Austrian-German believers in “a community of destiny and blood” that were specifically formulated in September 1914 by the German chancellor, Theobald von Bethmann-Hollweg. Yet at no time during Austria’s twenty-year independence did a majority of her people favor merger with Germany, nor even in the post-*Anschluss* plebescite held under Nazi jurisdiction, when 99.7 per cent voted yes. Closer links with Germany that might have helped Austria’s truncated economy after World War I were blocked by the Allies; in the 1930s, when she was economically viable, the

Western powers abandoned her to Hitler, whom they tempted with concessions so as to nudge Germany eastward against the Soviet Union. In support of these somewhat familiar propositions, Poltavskii cites from published documents and a wide range of secondary works. His final and most original chapter, based on materials in the archives of the Austrian resistance in Vienna, describes the Nazi purges of political and labor leaders and Jews and the beginnings of an opposition movement until August 1939. Poltavskii pleads repeatedly for Austrian scholars to abandon what he alleges to be their fact mongering and to concentrate on the role of the working class and the "treason" of the national bourgeoisie in the face of Nazi aggression. Despite some rigidity on questions that still require investigation, Poltavskii has produced a useful synthesis that places the *Anschluss* in a wide European and even global context.

Gerhard Botz adopts a more limited frame of reference than Poltavskii and concentrates upon the stages by which Austria was administratively and juridically reorganized after the Nazi takeover. Botz uses documents in the National Archives in Washington, the Deutsches Zentralarchiv in Potsdam and other repositories, Viennese newspapers, unpublished Austrian doctoral dissertations, and numerous specialized studies. His volume includes the texts of sixteen hitherto unpublished administrative memorandums written in 1938 by top Nazis. Botz believes the Nazis always strived for *Anschluss* but had no clear-cut plan for amalgamation when Austria fell to them. The reorganizational pattern they embodied in the so-called *Ostmarkgesetz* of April 14, 1938, "marked the pinnacle of centralizing tendencies in Germany and simultaneously a shift toward trends in an opposite direction." According to Botz, Nazi administrative measures were usually decided not upon any principled basis but through power struggles among Austrian and German Nazi leaders that were tolerated by Hitler. Botz concludes that the coming of World War II halted the further assimilation of Austria completely into the "Altreich." His book is slim but compresses much research and reflection. It will be indispensable for future microlevel studies of the Nazi occupation of Austria.

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JEAN-FRANÇOIS BERGIER. *Naissance et croissance de la Suisse industrielle*. (Monographies d'histoire suisse, volume 8.) Bern: Francke Éditions. 1974. Pp. 170. 18 fr. S.

Bergier, holder of the chair of history at the Swiss Institute of Technology in Zurich and secretary

general of the International Society of Economic History, presents a chronological overview of the industrial development of Switzerland. The first half of his booklet deals with the origins of industries in the Middle Ages and their evolution to the end of the eighteenth century. The second half is concerned with the industrial revolution of the early nineteenth century and the Swiss participation in and contribution to the industrialization of the world since then. A five-page "bibliographical orientation" concludes the work.

The growth of industry in Switzerland has always been determined by a lack of industrial raw materials, difficulty of access to cheap ocean transportation, and the necessity of transforming imported materials into goods of high value through specialized labor, streamlined organization, and inventive imagination.

From the Middle Ages to the early twentieth century various branches of the textile industry dominated the scene. The need for textile machinery gave birth to the machine industry in Switzerland in the first half of the nineteenth century. Switzerland followed its own course in the Industrial Revolution, directly behind England but ahead of the other European nations.

Bergier notes that the centers of industrial revolution in Switzerland were all Protestant. Practically everybody in those regions knew how to read and write. The working class was never an exploited illiterate mass. Swiss industries broke out of the narrow confines of their small country by opening branch establishments in other parts of the world on a scale unequaled by other countries except perhaps by the United States.

The structure of Swiss industry has always been fragile. It rests on a very narrow basis. Nevertheless it has steadily progressed through the centuries. Bergier thinks that this remarkable achievement is due to the fact that Swiss industry has constantly and vigorously been supported by a number of human values, namely dynamism, know-how, and integrity. His readable little book, though clearly designed for the general reader (whoever that may be), provides enough insights and observations to make it attractive to the economic history expert.

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CLEMENTE FUSERO. *The Borgias*. Translated from the Italian by PETER GREEN. New York: Praeger Publishers. 1972. Pp. 352. \$12.50.

E. R. CHAMBERLIN. *The Fall of the House of Borgia*. New York: Dial Press. 1974. Pp. xxx, 347. \$10.00.

The first of these two latest studies of the much-studied Borgia family can justly be classified as

biographical history. It belongs, that is, to the same genre of biographical writing as Giovannantonio Campano's life of Braccio da Montone in the fifteenth century and Pasquale Villari's life of Machiavelli in the nineteenth. It seeks not only to re-create the character and actions of its heroes, but also to elucidate a historical problem. It is concerned not only with individual men, but also with the times they lived in. And the principal Italian as well as English-language historical reviews erred in barring it from their review columns when it first appeared in 1966.

To be sure, the author's bibliography is not always up-to-date. Fusero relies on Henri Daniel-Rops rather than Hubert Jedin for church history, and on Giovanni Soranzo rather than Garrett Mattingly for diplomatic history; and he seems never to have heard of Eugenio Garin or Felix Gilbert. Moreover he knows nothing of such new historical subdisciplines as psychohistory, institutional history, and family history. Consequently, he often attributes the actions of his heroes to such now antiquated "causes" as "the burden of passion . . . accumulated in Rodrigo's breast" (p. 111) and Pedro Luis's "Borgia blood" (p. 139); and he fails to find any other bond among the members of the family—or among those of the other great clans of their age—than natural affection. Still, he knows the age fairly well, thanks in part to the careful preparation required for his two previous biographies of Leonardo da Vinci and Cesare Borgia, and he takes great pains to integrate his particular subject into the general context of Renaissance Italy. Thus the Borgia popes appear as the continuers of the policies of their predecessors since the Great Schism. Calixtus III's conflict with Alfonso I is shown to be a consequence of the complicated Aragonese succession in Naples. Alexander VI's patronage of the arts is attributed to the new position of Rome as one of the capitals of Renaissance culture. Moreover Fusero draws his information from all the major and many of the less well known contemporary sources: chronicles, diaries, diplomatic correspondence, medallions, paintings, papal bulls, private contracts, and at least some of the forty-eight volumes of the unpublished acts of Calixtus III. He also evaluates his sources as he uses them: he realizes that Burchard is a "crusty pedant" (p. 203) whose testimony is less reliable than that of the Florentine ambassador (p. 235), that the Este ambassador gives a more accurate description of Lucrezia's wedding than does the "libellous Infessura," and that Machiavelli's *Descrizione del modo* often contradicts what Machiavelli himself had written about the murder of Cesare's enemies in his reports to his superiors in Florence. True, Fusero has a few personal prejudices. He is

resentful of the pushy Spanish *conversos*. He hates the House of Aragon. And he is convinced that Conciliarists are fundamentally anti-Catholic. But he draws from his account a number of significant historical theses. Calixtus, he points out, inaugurated the pattern of papal nepotism that was to last well into the following century. Cesare's later conquests were greatly facilitated by the reputation he built up for just and efficient government in the lands he had previously conquered. Alexander finally made the Roman Church independent of the Roman barons, initiated a number of progressive reforms into the administration of the Papal State, and inaugurated the rebuilding of Rome that was soon to make it a monument of urban planning (pp. 267–68). Some of these theses can stand further testing—for example, that the duchy of the Romagna was a "model state." But all of them have the merit of obliterating most of the remaining traces of the centuries-old "Borgia legend."

Chamberlin's book, on the other hand, can best be characterized as biography *tout-court*, or, to use the Renaissance term, *de viris illustribus*. It seeks not to answer a historical question or to correct a historiographical error, but to present its heroes for the entertainment and moral edification of its readers. The title notwithstanding, it gives no more than a page to the history of the family after the death of Lucrezia, and it says nothing about the Spanish branch that lasted until the eighteenth century. It disposes of the whole subject of the growth of the papal bureaucracy—as if Peter Partner and Jean Delumeau had never written a line—with a few vague metaphors: "so enormous that its functioning was a daily miracle," "a species of dinosaur lumbering along" (p. 59). It overlooks entirely such related historical problems as the recrudescence of millenarianism, the structure of the Renaissance state, and the effect of the foreign invasions. It appeals to metahistorical causes to explain historical events, for example, to "that curious irony which was to attend all Borgia affairs" (p. 80) and the "quality of refined evil" and "alien ancestry" of that villain of villains, Ferrante of Aragon (p. 113). It dismisses the confrontation of Alexander and Savonarola, *pace* Donald Weinstein, in a few lines (p. 72), and it outdoes Machiavelli in reducing the Church to a purely secular institution: "The Cardinals," it maintains, "entirely abandoned the last pretense of exercising a spiritual office" (p. 126).

But even as biography, this book is not entirely successful. It portrays Rodrigo at one point as a skilled and calculating politician and at another as "essentially a man of moods and emotions, ever swinging from one extreme to another" (p. 93). It reduces Alfonso d'Este to a simple combination of

two passions, one for whores and one for guns, and it ascribes his friendship with Cesare solely to their common interest in artillery. And what little space it gives to the analysis of its heroes' character is buried in page after page of narrative about their *gesta*. It thus adds nothing and omits much from what is already in Fusero's book, which is not mentioned. It probably should be read not as a work of biography, but as "a story of murder, lies, hatred, and passion for power"—which is what the jacket cover claims it to be.

ERIC COCHRANE
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ANDRÉ ROCHON et al. *Les écrivains et le pouvoir en Italie à l'époque de la Renaissance: L'Arioste, Bernardo Dovizi da Bibbiena, Machiavel, l'Académie Florentine, Giambattista Giralaldi Cinthio*. (Centre de Recherche sur la Renaissance Italienne, 3.) Paris: Université de la Sorbonne Nouvelle. 1974. Pp. 325. 59 fr.

The five studies here continue Rochon's 1973 volume in this series and deal with various aspects of literary expression between 1508 and 1561 at Florence, Ferrara, and Urbino and with the increasing control that the princes of the time were gaining over cultural matters. All are useful and have much merit.

Dominique Clouet, "Empirisme ou égotisme: la politique dans la 'Cassaria' et les 'Suppositi' de l'Arioste," shows Ariosto obligated to celebrate the regime of Alfonso I d'Este during its difficult early years while at the same time determined to give a candid and for that reason cheerless picture of contemporary Ferrarese life. A close analysis of Ariosto's texts and of their antecedents in Roman comedy and elsewhere, along with an adequate though sparsely documented discussion of the times, demonstrates how he walks his tightrope. Anna Fontes-Baratto, "Les fêtes à Urbino en 1513 et la 'Calandria' de Bernardo Dovizi da Bibbiena," uses a much greater variety of materials to show how Dovizi went about producing a play that served both as the capstone of a celebration of Rovere power and, in its promotion of the Florentine as the national literary language, as an episode in Dovizi's lifelong service to the Medici. These two essays are sound interdisciplinary work with a reasonable purchase on the secondary historical literature and real contributions through the method of literary critique.

Marina Marietti, "Machiavel historiographe des Médicis," is a thoughtful study of Machiavelli's efforts, in particular those in the *Histories*, to admonish the family that he had to accept as his patrons, but it suffers from the imposition on the material of a modern conceptual frame. I find it much less likely that Machiavelli was

concerned about "the writer" and "authority" than about his own working self and Pope Leo X, or about the abstraction "power" so much as about the Principate of Augustus as he had abstracted it from Roman literature. Marietti works entirely from the secondary literature and documents published therein. Michel Plaisance, "Culture et politique à Florence de 1542 à 1551: Lasca et les Humidi aux prises avec l'Académie Florentine," continuing a study that appeared in the 1973 volume, is based on manuscript material—alone among the studies in this collection—that shows the academy during these years strengthening its hold on literary culture for the purpose of promoting the new absolutism. It is fair to describe the documentation as elegant and the work as one deserving separate publication in expanded form. In his discussion of persons and motives Plaisance shows a weakness for psychological terminology, but he seems to take no school seriously enough to interfere with his exposition.

The first four of these studies illustrate humanists functioning as such, to be sure under difficulties. Guy Lebatteaux, "Idéologie monarchique et propagande dynastique dans l'oeuvre de Giambattista Giralaldi Cinthio," shows humanism reduced to a mere catalog of *pièces justificatives* suitable for any purpose, but he retains a sympathy for Giralaldi, who in Italy in those days saw fit to sacrifice "la définition ontologique de l'homme à sa destination civique" (p. 309), in advance of so many other men of letters. Rochon's work as editor shows here a noteworthy coherence, for the earlier studies in the collection do much to illustrate and explain Giralaldi's peculiar development. But the goal of coherence may be approached at the expense of other advantages. There is much of the rigidity of the *problématique* about these studies, of questions asked that serve in the end to make more and not less obscure the thoughts of men long gone.

AVERY ANDREWS
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RAFFAELE CIASCA. *Aspetti economici e sociali dell'Italia preunitaria: Saggi*. (Studi di storia moderna e contemporanea, 4.) Rome: Istituto Storico Italiano per l'Età Moderna e Contemporanea. 1973. Pp. x, 457. L. 8,000.

The literature on the problems of southern Italy is constantly being increased by both Italian and foreign scholars. Since the eighteenth century, historians, economists, and sociologists have been studying this part of Italy and its population in considerable detail. Three of the five essays collected in this volume discuss the economic and social problems that have been important in the

development, or lack of it, during modern times of southern Italy. The first and the longest essay (pp. 9-250) reviews the various land reclamation projects planned and initiated, though mostly never completed, under the last Bourbon rulers and the attempts to deal with the problem in the early 1860s, immediately following unification. Social changes and stratification in the eighteenth and early nineteenth centuries are analyzed in the other two studies. The evidence presented by Ciasca refutes the image of a static society. Rather, it indicates that a high degree of mobility and advancement among the bourgeoisie and deterioration for the peasants altered the social structure of southern Italy during these years.

The fourth essay deals with the economic development of Lombardy between 1800 and 1870. This is an offshoot of Ciasca's earlier study, *L'origine del programma per "l'opinione nazionale italiana" del 1847-1848*, published in 1916. In both, the degree to which frustration with the economic policies of Austria in Lombardy influenced the Milanese middle class to support the Risorgimento is well documented. The last essay briefly reviews the importance of Melfi in Basilicata as a trading center in the Middle Ages and particularly its commercial relations with Florence.

Raffaele Ciasca, who retired in 1973 as head of the Istituto Storico Italiano per l'Età Moderna e Contemporanea, published these studies between 1923 and 1963 in different journals. To have them available in one volume is indeed fortunate. While much of what is presented in them has been integrated in other works, they remain important basic reading, together with the older studies by Sidney Sonnino, Leopoldo Franchetti, Pasquale Villari, Giustino Fortunato, and Gaetano Salvemini, for anyone wanting to study the southern problem in Italian history.

EMILIANA P. NOETHER
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MARCELLA PINCHERLE. *Moderatismo politico e riforma religiosa in Terenzio Mamiani*. (L'Età del Risorgimento: Studi e testi, 9.) Milan: Dott. A. Giuffrè Editore. 1973. Pp. 238. L. 3,400.

This volume, in the useful series directed by Professor Ghisalberti, should prove especially suggestive at this time, for the cumulative effect of recent studies into the limited social change brought by unification, the disarticulated economy of nineteenth-century Italy, the ambiguous machinations of the Cavourians, and the restricted part played by the populace have all served to increase the sense both of miracle and of disillusionment that contemporary Italians also felt in their own Risorgimento.

Much of the answer to the question thus revived of how Italy was united at all, of how so much could be accomplished yet so much left undone, lies in an understanding of the moderates who for a generation or more dominated Italy's intellectual, economic, and social life. Terenzio Mamiani was one of the most thoughtful of these. Born in the Papal States in 1799, he became a patriot whose enthusiasm for liberal progress drew him from philosophy to politics, sent him into exile as a young man, and led him to an honored place among Cavourians (minister of education in 1860, then professor, ambassador, and senator) until his death in 1885.

Most of the documents published here, the major part of the book, were written between 1830 and 1850; and they are intelligently summarized in a long introduction that might do more, however, to put them in the larger context of Mamiani's voluminous work.

His favorite theme was the regeneration of Italy, and his faith in that propels the long sentences of his dignified, academic prose from discussion of the land and the Italian language to proposals for works of charity and specific comments on taxes and constitutions. He was unaware of defending the interests of the propertied and unembarrassed by candid paternalism toward the lower classes. The spiritual regeneration he envisioned depended more on historical progress than revolution. Yet his faith in the power of education and of good example, of societies for agricultural improvement (he founded one in Pesaro), and of a free press was vibrant and courageous. All of this, like his detailed plans for elaborate welfare measures, was closely tied to his desire for a religious renewal that would reconcile the Church and progress.

The limits of the liberal dream—and the characteristic failure to understand what industrial society required—stand in sharp relief against a diffuse confidence. Still, his perceptive understanding of why other people's utopias were impractical and the very real fervor of his broad and generous patriotism represent the narrow circles of Italian bourgeois culture at their best and most dynamic.

RAYMOND GREW
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RENATO TREVES. *La dottrina sansimoniana nel pensiero italiano del Risorgimento*. (Pubblicazioni dell'Istituto di Scienze Politiche dell'Università di Torino, volume 29.) 2d ed.; Turin: Edizioni Giappichelli. 1973. Pp. 168. L. 2,500.

This book was first published in 1931. Added is the essay, "Saint Simonism and Italian Thought in

Argentina and Uruguay," a survey of the diffusion and the impact of the teachings of the French master, the count of Saint-Simon (Claude Henri de Rouvroy), mingled, as they were, particularly during the critical years 1838-48, with the ideals connected with the names of Giuseppe Mazzini and Giuseppe Garibaldi. Treves undertook the study in the 1930s while a refugee from fascism in South America. Also included is an article by Guido Maggioni, "Saint-Simonism in the Lombard Reviews: 1825-1848," representing additional research Treves had been unable to pursue, presumably because of the increasingly repressive climate in Italy. A note of urgency surrounds the publication because of the appearance of Francesco Gentile's *Saint-Simon in Italia* (1969), an inquiry initially printed in France. Viewed in its totality and broader sweep, from the reflections of Saint-Simon to the theoretical elaborations of his French disciples and Italian interpreters, Saint-Simonism, according to Gentile, evolved from a mood to a doctrine that, its apparent inconsistencies and naiveté aside, stirred the conscience and excited the imagination of many Italian patriots and reformers of the Risorgimento. It proved to be a valuable instrument as they went about confirming and revising where necessary the ideological heritage of the Enlightenment to meet Italy's special needs.

Treves disagrees with so positive an evaluation. He returned to the 1931 version and presents it here in its original form. In so doing he is insisting that it is highly speculative to assign Saint-Simonism a presence in Italy comparable to that of a genuine and coherent political and social philosophy, one which solicited and commanded implementary action in its behalf. Maggioni's examination of the Lombard periodicals is intended to reinforce Treves's judgment. Though Saint-Simonism may have constituted a school of thought, its precepts, advanced through volumes, reviews, and lectures, generated an ambivalence among the people it reached, which denied it an organic role. It appealed to those who had become disillusioned with the underlying assumptions of the Enlightenment—reason, natural law, and progress. Saint-Simonism also was attractive in the post-1789-1815 period to leaders and groups interested in the reassertion of the authority of the state, institutional refurbishment, who sought for social solidarity and religious revival. The cause of nationality that it championed, along with its concern for the emancipation of women, and the hopes expressed for a new form of associational living, epitomized in "the New Christianity," drew its number of admirers.

Simultaneously, however, most of those who had been impressed found much that was ob-

jectionable and downright ludicrous. The notion of compulsory service to society based on individual capacity and monetary rewards made dependent upon personal labor, the proposed elimination of the right of inheritance, the threat implied to private property, the rejection of representative government in its modern sense, the disavowal of violence, not to mention romantic recollection of the Middle Ages and the voice it raised for women's rights, exposed the impractical and far-reaching aspects of Saint-Simonism to unrelenting criticism and to ridicule. It is significant that personalities as diverse as Carlo Cattaneo, Antonio Rosmini, and Giandomenico Romagnosi immediately recognized the humane foundations of Saint-Simonism and just as instinctively rebuked most of its novel features as "ravings" (*dellini*), a revulsion shared by many commentators in Argentina and Uruguay. When socialism arrived on the scene, it absorbed whatever collectivist energies and utopian tendencies that previously had been identified with Saint-Simonism.

If Treves's pioneer effort can still stand on its own merits, it, nevertheless, suffers from being dated since it does not consider the more recent investigations into the subject of Saint-Simonism in Italy carried on by Delio Cantimori, Galante Garrone, Franco della Peruta, and Adolfo Omodeo. Moreover the author's suggestion that Mazzini's principle of nationality and his theme of "God and the People," to cite two illustrations, had Saint-Simonian overtones, together with Treves's firsthand observations concerning the pull Saint-Simonism exercised in South America, is evidence of the fame and survival value that Saint-Simonism retained in spite of its deficiencies as a movement committed to the remaking of man and society. Really, there is no contradiction between Treves and Gentile.

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TONI CERUTTI. *Antonio Gallenga: An Italian Writer in Victorian England*. (University of Hull Publications.) New York: Oxford University Press, for the University of Hull. 1974. Pp. viii, 204. \$13.00.

Antonio Gallenga was certainly an interesting if not an agreeable person. His most famous escapade was his journey to Sardinia in 1833, backed by Giuseppe Mazzini, to try to assassinate King Charles Albert. The story, when it became widely known in the middle fifties, caused both conspirators embarrassment; but, whereas it was neither surprising nor important that Gallenga had compromised himself, Mazzini's reputation for saintliness suffered then and subsequently. Gallenga's own historical significance lies largely in

his work as foreign correspondent and leader writer for the *Times* from 1859 to 1884. By the time he was sent to Italy by the newspaper, he had become a supporter of Sardinian initiative in the Italian movement. He covered the Expedition of the Thousand and encouraged the British public in its adulation of Garibaldi in 1860. With reservations Gallenga approved the regime that governed Italy after unification. He played a number of other roles during his career. He spent three years (1836-39) in the United States, meeting a good many brahmins and gathering material for some vivid memoirs. He was professor of Italian at University College London. He represented Sardinia at the Frankfurt Assembly of 1848, and was a member of Italian parliaments. He eventually set himself up as a landowner in Wales.

Dr. Cerutti does his best for Gallenga as a writer, but the author has to admit that Gallenga's more specific literary efforts were of poor quality. His journalism, on the other hand, was skillful and effective, even if it apparently always had to be "translated." It becomes clear from this biography that Gallenga, like many notable journalists, was a difficult, opinionated, and unreliable individual. Crabb Robinson regretted having engineered him into the Athenaeum; J. T. Delane said he "would quarrel with everybody in a month" if appointed to run the Paris office of the *Times*; his indiscretions troubled friends and employers alike. Cerutti, however, while citing much of the damaging evidence, continues to treat his subject with grave respect. The book makes available some new material and will be found reliable and useful. But the author's lack of subtlety in discussion of political questions seriously weakens this biography.

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FRIEDRICH ENGEL-JANOSI. *Vom Chaos zur Katastrophe: Vatikanische Gespräche, 1918 bis 1938. Vornehmlich auf Grund der Berichte der österreichischen Gesandten beim Heiligen Stuhl.* Vienna: Verlag Herold. 1971. Pp. 320. Sch. 268.

This is an account of Vatican diplomacy from the chaos created by the Paris peace treaties of 1919 to the catastrophic events of 1938 that foreshadowed the Second World War. It is based primarily on the reports of two Austrian diplomats to the Holy See, Ludwig von Pastor and Rudolf Kohlruss. There are no great surprises or major revelations; that the Holy See was sympathetic to conservative and authoritarian governments and fearful of communism, and that Pope Pius XI and his secretary of state, Eugenio Pacelli, were friendly to Austria and Weimar Germany have been known all along.

Still some details are interesting: Pacelli's disclosure of the papal encyclical, *Mit brennender Sorge* (on the condition of the Catholic Church in Germany), to Kohlruss; the special conditions for keeping it secret, which, in the end, were not observed; the German-Austrian struggle for Santa Maria dell'Anima.

The study ends with the *Anschluss* of March 1938 and is followed by a short section on Pius XII, elected pope in February 1939. Professor Engel-Janosi explains Pius XII's policies during the war, especially toward the Third Reich, and clarifies the pope's efforts on behalf of the Poles, the Serbs, and the Jews. Pius's policy toward Nazi Germany was largely determined by the attitudes of the German bishops, the majority of whom favored peaceful accommodation with the government and feared the loss of their flock's allegiance. The pope's silence toward the persecution of Poles, Serbs, and Jews can be explained, according to the author, by Pius's personality, his inability to use "fiery language," his fear that speaking out would make matters worse, and his concern about the fate of forty million German Catholics. In the case of the Jews, Engel-Janosi asserts that the Catholic Church saved between 700,000 and 850,000 Jews, more than all other relief organizations combined, and he points to the fact that ninety per cent of all Jews in Rome survived, while in the occupied Netherlands, where the clergy and laity resisted the Nazis seventy-nine per cent were deported.

This is a useful book illuminating the details of Vatican diplomacy and personalities during a difficult period.

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IRMGARD WILHARM. *Die Anfänge des griechischen Nationalstaates, 1833-1843.* (Studien zur Geschichte des neunzehnten Jahrhunderts, 5.) Munich: R. Oldenbourg. 1973. Pp. 274. DM 52.

Professor Wilharm's monograph studies the attempt to place a foreign (Bavarian) dynasty on the newly created nation-state of Greece in the 1830s. Relying chiefly on documents found in the Staatsarchiv in Munich and Foreign Office documents from London's Public Record Office, he examines the interrelationship among the Bavarian administrative and military monarchical entourage, introduced with Otho Wittelsbach into Greece; the native Greek social and political leadership; and the three great powers (England, France, and Russia), which, by the Treaty of London in 1832, primarily established the infant state. Wilharm seeks to explain the failure of this experiment in foreign monarchy.

The author sees as the primary dilemma for the king's regents, and after his majority Otho himself, the choice between following a rational policy of economic development and stabilization or expending Greece's meager resources on a popular irredentist foreign policy. Since the legal status of the regency was unclear, the ultimate decision lay with Otho when he assumed direct control of the government in 1837; and he unwisely chose the latter policy achieving neither the "Great Idea" nor stabilization and support within the country. Yet Wilharm also states that the feasibility of the alternative policy of abjuring the "Great Idea" "cannot be decided with certainty" (pp. 252, 260).

The confrontation between native Greeks and the Bavarian dynasty showed itself conspicuously in the struggle for a constitution promised by the Treaty of London but adamantly opposed by the regents, the king, and the king's father, Ludwig of Bavaria. The constitution became a rallying cry for the opposition, both formal parties and gangs of brigands. However, Wilharm sees as equal requirements for successful government extensive participation of Greek nationals, land reform, and a successful expansive foreign policy. The monarchy was unwilling or unable to pursue any of these sufficiently and hence failed to establish itself in Greece. The result was the revolution of 1843, bringing Greece a constitution against the wishes of the king and leaving the monarch a stranger in his own land.

Besides archival material Wilharm relies heavily upon contemporary and subsequent secondary works for many of the aspects of this period. He uses quite frequently John A. Petropoulos's detailed book *Politics and Statecraft in the Kingdom of Greece, 1833-1843* (1968) for much of his analysis of the political parties of the time, although he disagrees with him on a few particulars, such as the relationship between the parties and Greek brigandage (p. 204). Still the monograph stands by itself as a thoughtful, balanced persuasive view of early Greece from the Bavarian perspective.

FREDERICK B. CHARY
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HEINZ RICHTER. *Griechenland zwischen Revolution und Konterrevolution (1936-1946)*. With a foreword by KOMNINOS PYROMAGLOU. [Cologne:] Europäische Verlagsanstalt. 1973. Pp. 623.

This is a dissertation of truly Germanic proportions for which the reader, if he is to do it justice, had better set aside his summer vacation. For whatever he gives up he will be rewarded with a finely woven narrative that probes all the major issues without, surprisingly, losing itself in irrelevant detail. The fact is that there are so many such issues—or so many times that the same ones keep

reappearing—that extensive treatment is justified, though the author might spare us some of the instances where he feels compelled to point out that other writers were wrong.

Richter has used the recently published British and American documents, the German occupation records, the voluminous firsthand testimony of Churchill, Eden, Macmillan, Myers, Woodhouse, and others on the British side, and all the available Greek sources. He tracks down many hitherto unanswered questions surrounding the Greek resistance, the factional differences within the Communist-led and non-Communist liberation organizations, their struggle against each other, and especially the role of the British in dealing with both. He gives a clear picture of the strengths and weaknesses of the Greek Communist party and how it faced the critical issue of national versus Moscow-oriented interests and policies.

Apparently no one can be neutral about Greece and Greek politics, and Richter has a definite point of view. The heroes are the Greek people in general and the left-of-center democratic resistance leaders in particular—especially Komninos Pyromaglou, who contributes the book's preface. The villains are King George II, fascist-minded military men, the old parties and politicians, the German occupiers and the British occupiers who replaced them. The theme of common German and British interest runs through the book from the installation of the Metaxas dictatorship in 1936 to the German withdrawal in 1944. British policy is the "external factor" forever lurking behind the scenes, and sometimes taking the center of the stage, pursuing its own ends at the expense of Greek democracy. One can take the author's interpretations at face value or apply whatever discount one chooses. In any case there is no denying the solid scholarship that has gone into digging out and assembling the material. Historians will not stop writing about this period. It has too much interest and too much significance for Greek politics for the indefinite future. But any who decide to do so will have to contend with the facts and opinions that Richter has put together in this book.

JOHN C. CAMPBELL
Council on Foreign Relations

M. BERZA et al., editors. *Relatii româno-bulgare de-a lungul veacurilor (Sec. XII-XIX): Studii* [Romanian-Bulgarian Relations in the Course of the Centuries (12th-19th Centuries): Studies]. (Academia de Științe Sociale și Politice a Republicii Socialiste România, Institutul de studii sud-est europene.) Bucharest: Editura Academiei Republicii Socialiste România. 1971. Pp. 431. Lei 28.

The first of a proposed two-volume collection of studies on Romanian-Bulgarian relations concerns

itself almost exclusively with problems of economic and political history. The contributors are all distinguished students of Bulgarian and Romanian history, and the quality of the studies reflects the expertise of the authors.

There is little topical correlation among the various contributions that are devoted to such diverse matters as the role of the Wallachians in the establishment of the Second Bulgarian Empire, that of the Bulgarians of Wallachia in the revolution of 1848, and the "Song of Plevna" in southeast European folklore. Most of the studies break little new ground either in method or in providing new archeological or archival data.

Borislav Primov's study on the Second Bulgarian Empire restates the generally accepted view of Wallachian participation in the establishment of that empire. Ivan Duicev's article on Peter Parchevich's efforts to organize a war of liberation against the Turks and Traian Ionescu-Niscov's description of the national liberation movement of the Bulgarians in the nineteenth century are useful summaries of the existing literature. Vera Mutafchieva's and Al. Vianu's joint study on Pazvantoglu's rebellion and rule in northern Bulgaria is a methodical and detailed review of an abundant collection of published primary and secondary sources. This is true also of C. Velichi's article on the Bulgarians and the revolution of 1848.

Much more original are the contributions on social and economic history. The work of D. Angelov and Stefan Stefanescu on the comparative socioeconomic development of Bulgaria and Wallachia in the thirteenth and fourteenth centuries stresses parallel phenomena in two areas with comparable social structures and economic and political problems. Bistra Tvetkova complements that study in an excellent account of economic relations between the Bulgarian and the Romanian areas on both sides of the Danube in the sixteenth century. The multiauthored article devoted to the status of the Bulgarian immigrants in Wallachia and Oltenia in the nineteenth century adds a further dimension to the socioeconomic relations of the two peoples.

The only truly original and as such most valuable contribution is by Adrian Fochi. The author analyzes the variants of the "Song of Plevna," which emerged after the battle of 1877-78 in various parts of Eastern Europe. His concern is mainly literary and folkloristic but the *explication de texte* is important also to the historian of Romanian-Bulgarian relations. The volume as a whole is a significant contribution to the history of southeastern Europe.

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R. J. W. EVANS. *Rudolf II and His World: A Study in Intellectual History, 1576-1612*. New York: Oxford University Press. 1973. Pp. xi, 323, 16 plates. \$24.00.

During the past few years a number of important books have appeared on the cultural life of that rather mysterious period between the Council of Trent and the Thirty Years' War, an epoch marked by the climax of witchcraft persecutions and a flowering of the occult arts and magic, of alchemy and astrology. Evans's work on the court of Emperor Rudolf II and the personalities drawn to Prague through his patronage provides a fine addition to this body of scholarship. Even more important, it offers the beginning at least of a new synthesis, a revaluation of traditional interpretations that were more concerned with reading the Enlightenment backward into the Renaissance than with trying to understand the highly complex, often eccentric reactions of educated Europeans to the spiritual insecurity that resulted from the confessionalization of life in the sixteenth century.

The evidence Evans brings to this task is formidable. The bulk of it takes the form of brief discussions of some hundred or more leading figures of the age who appeared at one time or another in Prague or who corresponded with intellectuals there. The cross-section is well chosen, representing great Czech nobles whose patronage rivaled Rudolf's, courtiers, poets, printers, librarians, theologians, antiquarians, painters, alchemists, astronomers, and a few charlatans. Most of them were polymaths whose creative interests covered several of these categories at once. Evans follows Friedrich Heer's provocative hypothesis in presenting this intellectual community as a group of moderate reformers seeking a "third way" to truth through an occult humanism that could avoid the increasingly rigid orthodoxies of Rome, Wittenberg, and Geneva.

The main cultural tendencies of Rudolfine Prague—the mania for collecting, exaggerated symbolism in the arts and in ritual, cultivation of natural and experimental philosophy, irenic theology, exaltation of the imperial monarchy, and a preoccupation with the occult and magic—are brought together here not through any inner logic of their own, but through the spiritual yearnings of men who hoped to find a lost universal harmony by deciphering the secret language of nature and by discovering the mysterious links they assumed bound natural phenomena organically to an underlying reality. The resulting mood was pessimistic, pansophist, highly elitist, almost gnostic. It was also fragile, swept away in the end by the rise of a new religious orthodoxy and by the mechanistic world view of Galileo's successors. Perhaps the main contribution of Evans's fine and erudite book is to make this strange world around the

melancholy Rudolf historically believable. It is a must for every academic library.

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JAROSLAW PELENSKI. *Russia and Kazan: Conquest and Imperial Ideology (1438-1560s)*. (Near and Middle East Monographs, 5.) The Hague: Mouton, 1974. Pp. xii, 368. 90 gls.

This is an important book dealing with a problem too long neglected in traditional historiography: the construction of an imperial ideology not only as a justification of Moscow's territorial expansion but as a rationale for its transformation into a multinational empire. The middle of the sixteenth century was a watershed in the history of Muscovite institutional development. It was a time when, with the establishment of the tsardom, the creation of a new military service bureaucracy, the serious expansion into non-Slavic Muslim areas for the first time, and the elaboration of a Providential interpretation of its own history, Moscow took several giant steps toward the formation of a formidable autocratic state, which would sit astride and dominate the Eurasian steppe. Moscow's special position and pretensions within the Slavo-Turkic steppe and the consequences of its interaction with its Turkic neighbors deserve closer study. With the conquest of the Kazan khanate in 1552, Moscow not only gained control of an extremely important center of international trade and the rich agricultural lands of the khanate but it pacified the hitherto turbulent and distracting eastern border and opened the door to opportunities for dramatic expansion to the east and southeast. Muscovite bookmen seized the chance to draw appropriate imperial lessons from the event and to weave a web of justifications, which would at the same time enhance the prestige of the state.

Pelenski reviews and analyzes all of the pertinent literature and arguments of the period: the patrimonial and dynastic claims based on the activities and conquests of the Russian grand princes along the middle Volga as early as the ninth and tenth centuries, the intricate parallel *translatio* theories of political continuity linking Kiev, Vladimir, and Moscow on the one hand and the Volga Bulgars, Golden Horde, and Kazan khanate on the other, the right of conquest invoked as the occasion demanded in correspondence with various Turkic polities, and, perhaps most important, the Providential interpretation of the destiny of the House of Moscow as propounded by the Muscovite high clergy—to subdue and convert the “barbaric nations.” These arguments need not have been terribly sophisticated in order to be effective. At least, as Pelenski correctly notes, the Muscovite ideologues knew what they wanted and were able to

compose a coherent and impressive ideological framework for their goals, which was more than their adversaries, real or potential, were able to do. The author has produced a careful and important piece of work.

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M. I. SLUKHOVSKII. *Russkaia biblioteka XVI-XVII vv.* [The Russian Library in the 16th and 17th Centuries]. Moscow: Izdatel'stvo “Kniga,” 1973. Pp. 252.

S. P. LUPPOV. *Kniga v Rossii v pervoi chetverti XVIII veka* [The Book in Russia in the First Quarter of the 18th Century]. (Akademiia Nauk SSSR, Ordena Trudovogo Krasnogo Znameni Biblioteka Akademii Nauk SSSR.) Leningrad: Izdatel'stvo “Nauka,” 1973. Pp. 372.

The titles of these monographs suggest broad studies of two related subjects—the nature of libraries and the role of books in a society that was in the throes of a far-reaching intellectual awakening. As it develops, the authors have set themselves more modest goals. Slukhovskii's study deals mainly with how libraries were formed, operated, and structured in sixteenth- and seventeenth-century Muscovy, while Luppov's work centers upon the change of orientation that occurred in Russian book publication during the first quarter of the eighteenth century. Both monographs are continuations of earlier studies by the authors involving libraries and books in other periods of Russian history.

Slukhovskii's latest volume dwells at length upon book repositories controlled by the Russian Orthodox Church that were primarily monastic libraries. Over two hundred monasteries are cited, many of which contained sizable book collections. In seven tightly woven chapters the author describes the building complex and floor plan of such libraries and explains how their books were requisitioned, inventoried, loaned, and replenished. The wealth of detail that he has uncovered is astonishing. One almost has the feeling of being part of an official inspection party on tour of the Muscovite library system with an eye toward examining its operation and suggesting reform measures and lower operating costs. Even the responsibilities, performance records, and idiosyncrasies of Muscovite librarians are discussed and the subtleties of library terminology analyzed. But what ultimately emerges from this scholarly study is a heightened sense of the pervasive influence of the Orthodox Church upon the intellectual life of Muscovy and an appreciation of the clergy's dedication in building, preserving, and replenishing great book collections of the time. Slukhovskii has enriched his work with numerous supplementary

notes, photographs, diagrams, indexes, and a glossary. More attention to book titles found in particular libraries would have been welcome, but one must recall that this was not the primary purpose of the author and that such data are often lacking in the sources. The inclusion of a summary chapter would also have strengthened the book.

Luppov's study, in turn, is directed toward a broader circle of readers and is concerned with book printing, book markets, and the growth of libraries during Peter's reign. In six lucid chapters the author outlines particular problems of the Russian Enlightenment and the influence of the new schools on book buying and library development. We learn of the rapid transition that took place in the subject matter of books from emphasis on religious topics in the seventeenth century to interest in secular subjects of the eighteenth century. The author provides some striking tables showing the size and general content of some thirty private libraries and explains how the press, which had formerly been under Church control, came under government influence. Like education, the press was enlisted to promote change in the emerging Russian Empire that was clearly reflected in the subject matter of new books. Luppov acknowledges the strong influence of Peter's predecessors on many of his reform measures but assigns the tsar a high degree of responsibility for changing the trend of publication. Such change, he asserts, was made possible only by the existence of strong authority. This study is also well researched, organized, and annotated.

The data assembled by these authors bring into a view a side of Russian intellectual life that has largely been unexplored. While the Slukhovskii volume provides the clearest picture to date of the organization, operation, and physical attributes of Muscovite libraries, Luppov's work documents the relationship between book publication and government policy in vigorous style and in the process provides some delightful insights on the literary tastes of Russian society in the Age of Peter. Both monographs exemplify the kind of skillful archival research that Soviet historians are so well equipped to perform, and in this case some admirable results have been achieved.

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M. F. SHABAEVA, editor. *Ocherki istorii shkoly i pedagogicheskoi mysli narodov SSSR, XVIII v.-pervaia polovina XIX v.* [Essays on the History of Schools and Pedagogical Thought of the Peoples of the USSR, 18th-First Half of 19th Century]. (Akademiia Pedagogicheskikh Nauk SSSR, Institut Obschei

Pedagogiki.) Moscow: Izdatel'stvo "Pedagogika." 1973. Pp. 603.

This manual on the schools and the pedagogical thought of the Russian Empire during the eighteenth and early nineteenth centuries is intended mainly for research assistants, students, and teachers at Soviet pedagogical institutes. It is the work of a *kollektiv* consisting of thirty-nine authors, bibliographers, and editorial assistants. The advantage of such a large number of contributors is the expertise they are in a position to offer on a wide variety of subjects, while the disadvantage can be unevenness of treatment and the lack of a unifying theme and point of view.

As the editor quite correctly points out in her introduction, the history of education among the non-Russian peoples of the empire is a subject that pre-1917 scholars tended to ignore. This generalization applies, however, more to the eastern than to the western borderlands of the empire. The chapters in this manual on education prior to 1850 in Georgia, Armenia, Azerbaidzhan, Central Asia, and Kazakhstan can truly be considered an important contribution. In regard to the western borderlands, however, pre-1917 writers tended to have a better feeling for the concrete historical conditions and the traditional values and culture of this area than do the Soviet writers of our *kollektiv*. They are, it might be mentioned, particularly insensitive to the religious values of some of the pedagogical thinkers about whom they write.

The *kollektiv* devotes 386 out of 562 pages of text to Russian education and pedagogical thought. The discussions of Peter I's educational reforms and the pedagogical thought and activities of V. N. Tatishchev are particularly well done. Chapters on the early nineteenth century also contain materials of interest on such matters as specialized and women's education, technical schools, and pedagogical institutes and journals. Three chapters are on the contributions of the Decembrists to Russian education and the pedagogical views of V. G. Belinskii, A. I. Herzen, and N. P. Ogarev. Yet the ideas and activities of S. S. Uvarov, an educational administrator and theorist of some note and the minister of education from 1833 to 1848, are not seriously discussed by the authors of the *kollektiv*. Can so many pages on radical intellectuals be justified while someone like Uvarov is almost overlooked? Not if the primary concern is to present the history of nineteenth-century education in the empire as fully and accurately as possible.

Many students of the history of education will want to consult this work because of its useful bibliography. Only works published in the languages of the peoples of the USSR are included. This is regrettable, for there is also a valuable

scholarly literature in Polish and German on the subject matter covered in this volume.

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V. IA. LAVERYCHEV. *Tsariizm i rabochii vopros v Rossii (1861-1917 gg.)* [Tsarism and the Labor Question in Russia (1861-1917)]. Moscow: Izdatel'stvo "Mysl," 1972. Pp. 338.

Reading a Soviet book makes one think of Dr. Johnson's famous observation about a woman preaching, which he likened to a dog walking on its hind legs—"It is not done well, but you are surprised to find it done at all." Similarly, one is surprised to find that any book written in the obscurantist atmosphere engendered by the policies of the Communist party could have any merit at all. Laverychev's book has much merit, but it suffers from the grave defects often found in Soviet scholarship.

Prominent among the shortcomings is the polemic tone of much of the book. The author refers to a government commission studying the labor question as the "Octobrist-Black Hundred Commission," and he is much given to applying to Lenin's opponents such pejorative terms as "Menshevik-Opportunists," "Social-Chauvinists," and "pseudo-socialist parties" occupying "perfidious-renegade positions." One wonders how many more decades must pass before these ancient quarrels can be treated by Soviet scholars with the calm objectivity that is expected of a sophisticated historical science. The use of Lenin as the most authoritative source must be accounted as another serious defect; Laverychev baldly states in his introduction that "the most important methodological basis for a study of the policies of tsarism in the labor question is the work of V. I. Lenin," and indeed many sentences begin with the oracular phrase "V. I. Lenin pisl." Jargon intrudes at many points in the narrative, and one is conscious of a straining to fit historical reality into the Marxist mold (for example, in relating the shifts in labor policy to the progression from a "feudal" to a "bourgeois" monarchy). It is not difficult to imagine Laverychev himself yawning as he makes his obligatory bows in the direction of the party bureaucrats.

It is fortunate that the patient reader will find much of value here to compensate for the blemishes. The author, who is an established authority in the field, traces in rich detail the whole course of labor legislation in the prerevolutionary period. The findings of a seemingly endless parade of government commissions, the quarrels between government departments, the tensions between tsarism and the industrialists, and the disagreements

among the industrialists are all described at great length. The principle that evolved to guide the tsarist regime in its labor policy was that of "guardianship" or "trusteeship" (*popечitel'stvo*), at the basis of which lay the regime's conception of itself as a supraclass state with an obligation to care for all the social classes. With this notion to work from, the regime gradually and tortuously brought forth a series of measures beginning in the 1880s to do such things as regulate child labor, provide insurance for sick workers, and set up a system of factory inspection. The government never went as far as the more advanced Western states, and the author finds the whole guardianship policy fraudulent in that the tsarist policies had a "clearly expressed class character hostile to the people." It was in general a cover for "a cruel exploitation of the workers." Although the regime occasionally pressed the industrialists to make economic concessions to the workers for the sake of reducing political agitation, it never hesitated to use police and military measures to control the working class. These measures were harsh enough to stimulate the more enlightened industrialists, in the period just before World War I, to seek a more moderate policy from the government on the grounds that such severe repression speeded up the process of radicalization of the workers. The contradiction which the regime could never resolve was simply that of preserving autocracy while creating a labor code that would satisfy both the bourgeoisie and the working class. Laverychev ends his book rather bombastically with the statement that "the working class, led by the Leninist Party . . . achieved a socialist revolution, consistently and for all time solving the labor question in Russia." One wonders how an inhabitant of the Gulag Archipelago would comment on the "Leninist solution."

Laverychev has made extensive use of the great Russian historical archives. He quotes often from contemporary periodicals of various political persuasions. He does not, however, cite any Western scholars, several of whom have made important contributions to this field in recent years. Readers wishing to supplement this Marxist account may consult Reginald E. Zelnik's *Labor and Society in Tsarist Russia* (1972) for both its text and bibliographical comments.

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DAVID MACKENZIE. *The Lion of Tashkent: The Career of General M. G. Cherniaev*. [Athens:] University of Georgia Press, 1974. Pp. xix, 267. \$11.00.

For both the specialist in Slavic studies and the general reader of historical literature, publication

of a biography of General Mikhail Grigorevich Cherniaev is a significant literary occurrence. Through the public life of Cherniaev the reader can view the unfolding of historical developments of crucial importance to Russia, Central Asia, and Europe. Cherniaev was deeply involved in the conflict between the liberal reformers in the bureaucracy of Alexander II and the conservative-nationalist aristocrats seeking to recover influence lost during the so-called era of great reforms, the even more fundamental struggle of these same aristocrats to guard their prerogatives against a rising bourgeoisie, Russian imperial expansion into Central Asia and the consequent international problems that this expansion caused, the development of pan-Slavism as both the balm for a wounded aristocracy and a rationale for imperial expansion, and Russian involvement in the Balkans, which drew Russia into an ever-widening conflict with all of Europe. Cherniaev conquered Tashkent for Russia in 1865. Soon after he was recalled from his post in Central Asia for quarreling with his superiors and for exceeding his orders. Seen by conservatives as a martyr on the altar of "bureaucratic liberalism," he became the object of pan-Slavist adulation and joined their struggles against the liberal ministers, especially the minister of war, D. A. Miliutin. Prompted by his pan-Slavist friends and his own sense of nationalist mission, he accepted command of the Serbian army in Serbia's disastrous war with Turkey in 1876. He then helped his fellow pan-Slavists push Russia toward war with Turkey.

As MacKenzie concludes, Cherniaev's reputation as the darling of the conservative Russian nationalists rested partly on his pen. Like his American counterpart General Custer, General Cherniaev carefully cultivated his own image. His military reports, from Central Asia and later from Serbia, magnified minor encounters into major victories and blamed his defeats on the timidity or duplicity of his superiors. Constitutionally unable to follow orders, he was "an adventurer thrust forward by the age of imperialism, gaining glory by conquering the weak and backward. . . . His fame and reputation were enhanced beyond measure by Russia's psychological need for heroes, by its striving for equality with a West more advanced economically and technologically, by its efforts to achieve an exulted historic mission" (pp. 243-44). Indeed Cherniaev's career is a sorry tale of monomaniacal ambition and bureaucratic intrigue.

MacKenzie spares the reader none of the sordid details. Generally well written and thoroughly researched, his account of Cherniaev attempts to show how he was shaped by the events of his time, and how he in turn influenced the course of Rus-

sian history. MacKenzie's study, however, has several serious flaws. First of all, he focuses too narrowly on Cherniaev. For example, he frequently notes that Cherniaev's inner conflicts reflected the conflicts of the nobility in an era of rising capitalism, of a nobility jealously guarding its privileges and waning power. Yet he nowhere adequately analyzes this important class conflict; he does not show specifically how the struggle between the nobility and the rising bourgeoisie determined Cherniaev's activities. Nor does he give the general reader enough information on the inner workings of the tsarist bureaucracy, information vital to the nonspecialist who wishes to untangle the complex disputes between Cherniaev and his superiors. Throughout most of Cherniaev's career his major antagonist was Miliutin, Alexander's liberal minister of war who sought to reform and modernize the Russian army. Among other things, Miliutin attempted to make the field generals directly responsible to the ministry of war, a move bound to raise the enmity of Cherniaev and his fellow aristocratic field officers. To fully understand this dispute, the nonspecialist would have to know much more about Miliutin and the Miliutin reforms than the author reveals in this book. Moreover, MacKenzie gives the general reader few hints on how the ministerial system as a whole worked or failed to work in the face of an insubordinate adventurer like Cherniaev. MacKenzie does not specifically say whether Cherniaev owed primary responsibility during his Central Asian campaign to the ministry of war or to the ministry of foreign affairs. He should have at least explained that ministerial solidarity did not exist in Russia at this time; each minister reported directly to the tsar. The most important minister was the minister who held the tsar's confidence at any particular moment. This state of ministerial anarchy made it possible for an ambitious general like Cherniaev to exceed his orders, exploit differences between ministers or take advantage of the confusion of the tsar. The author throws around the names of ministers without indicating clearly what their relative positions to the emperor were, or where they might fit in the ministerial structure at the time of their conflict with Cherniaev. He simply fails to describe adequately the bureaucratic context of Cherniaev's disputes with Miliutin.

The most serious fault in this study, however, is the author's own liberal bias. This bias affects the way he treats Cherniaev's personal life as well as the way he interprets the significance of the general's career. He correctly describes the struggle between Cherniaev and Miliutin as a struggle between nationalist-conservative and liberal, but definitely joins the debate on Miliutin's side. He never resists the temptation to administer a gra-

tuitous slap at the bumbling, anachronistic Cherniaev—whom he characterizes as a throwback to the reign of Nicholas I, an impediment to progress and bureaucratic efficiency—whereas Miliutin comes through the narrative as the very breath of reason and progress. The most unsavory examples of the author's prejudicial treatment of Cherniaev are the recurrent references to the general's homosexuality. Either MacKenzie should have engaged in a thorough psychological study of his subject and shown how his homosexuality shaped his personality and public life, or he should have left the poor man's sex life alone. In a word, MacKenzie has not written a fair, unbiased, or objective account of General Cherniaev. One does not have to be an admirer of Cherniaev or a defender of obscurantism and Russian imperialism to notice that what MacKenzie does is to pose sources against each other and then choose the ones most critical of Cherniaev—usually the liberal sources. The author's bias, a bias that prompts him to believe the worst about his subject, seriously undermines the value of this study.

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V. S. POZNANSKII. *Ocherki istorii voozruzhennoi bor'by Sovetov Sibiri s kontrrevoliutsiei v 1917–1918 gg.* [Essays on the Armed Struggle of the Siberian Soviets against the Counterrevolution, 1917–1918]. (Akademiia Nauk SSSR, Sibirskoe Otdelenie. Institut Istorii, Filologii i Filosofii.) Novosibirsk: Izdatel'stvo "Nauka," Sibirskoe Otdelenie. 1973. Pp. 306.

While Western scholars have almost ignored the civil war in Siberia, except concerning Kolchak and Allied intervention, Soviet scholars have given this subject a great deal of attention. This is especially true since the Twentieth Party Congress in 1956 when historians were directed to correct distortions caused by Stalin's personality cult. Thereafter historical output greatly expanded and improved, but Western historians will never be fully satisfied with Soviet works so long as politics determines their general direction, content, and use of jargon. Yet, when one overlooks these annoyances, he can find examples of solid historical research in sources often inaccessible to Western scholars. Such is the case here.

V. S. Poznanskii's major theme is Siberia's importance in the success of the Bolshevik Revolution. Western historians generally stress Bolshevik weakness because of Siberia's lack of a proletariat. Exceptional is the unpublished dissertation of Russell E. Snow, "The Bolsheviks in Siberia, February 1917–March 1918" (1972), which emphasizes Bolshevik strengths, notably leadership and tac-

tics, compared with their competitors, and Siberia's importance to Lenin because of its much-needed grain. Poznanskii agrees, but concentrates instead on Siberia's military importance. He details the struggle against Ataman Semenov in Transbaikalia, an early front, and the decisive blow given Soviet power by the Czechs. Repeatedly Poznanskii condemns G. Kh. Eikhe, prominent military leader and memoirist (*Oprokinutyi tyl* [1966]), for blaming the defeat in mid-1918 on weak Bolshevik leadership. He defends these leaders and their instrument of power, Tsentsosibir' (Siberian Central Executive Committee), emphasizing that defeat resulted from the military superiority of their Allied-supported adversaries and middle peasant opposition to Soviet power. However, revolutionary forces prevented the anti-Bolsheviks from marching rapidly on Moscow and Petrograd and thus helped save the revolution. One wishes the author had examined the political struggle for Siberia, including dissensions among the Bolsheviks, in the critical weeks after November 1917.

Poznanskii's documentation and command of Soviet source materials are impressive. Students of the Soviet period will profit from his excellent twenty-five page introduction concerning the nature and location of documentation for this period. He includes another useful tool often absent in Soviet works, a lengthy annotated index of names, but omits a bibliography, a common Soviet practice.

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ALFRED ERICH SENN. *Diplomacy and Revolution: The Soviet Mission to Switzerland, 1918.* (International Studies of the Committee on International Relations, University of Notre Dame.) Notre Dame, Ind.: University of Notre Dame Press. 1974. Pp. 221. \$9.95.

Following the Bolshevik Revolution the fledgling Soviet state managed to establish missions in four countries: Germany, Sweden, Great Britain, and Switzerland. In this continuation of his study of the Russian Revolution in Switzerland, Alfred Senn attempts to shed light on how the Soviet Union viewed its place in the international sphere during the first year of its existence by closely examining the Swiss mission of 1918. Lenin himself took considerable interest in the mission, viewing Switzerland as an excellent place from which to disseminate information about the Russian Revolution and from which to gather information about socialist and worker attitudes. This, rather than diplomacy in any usual sense, was in fact the main task of the mission. After some false starts the

mission was finally established under Jan Berzin, a Latvian and friend of Lenin. With the assistance of a variety of Russians, Swiss, and others, such as the Frenchman Henri Guilbeaux, the Soviet mission set up several press and other organizations to gather information and, more important, to disseminate propaganda in favor of the Soviet regime. As a result of this and meddlings in Swiss political affairs, the mission and other Russian revolutionaries—their relation to the mission was often very unclear—drew fire from both non-socialist Swiss and foreign embassies, especially the French. Finally pressures from the now victorious Entente combined with growing Swiss uneasiness over their activities—especially those of Angelica Balabanova and others in connection with a major strike in November—led to the Russians' unceremonious deportation.

Senn has succeeded well in the task he set for himself: the detailed description of the mission, the various Russians who surrounded it, and their activities in Switzerland. As in previous works he has mined the wealth of sources industriously. The parade of characters sometimes becomes a bit bewildering, giving the text a disjointed air at places, but the prose quickens at other points. One could wish, however, that Senn had made a greater effort to tie the Swiss mission into the broader spectrum of international developments of the era and to tie it to Soviet expectations and thinking about international affairs, imminent revolution, and relations with other socialists as well. Nonetheless he has written a fine account of one of the first episodes in the development of the novel mix of diplomacy and revolutionary activity that has constituted Soviet foreign policy.

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S. N. SEMANOV. *Likvidatsiia antisovetskogo kronshadtского miatezha 1921 goda* [The Liquidation of the Anti-Soviet Kronstadt Revolt of 1921]. (Akademiia Nauk SSSR, Nauchnyi Sovet po Kompleksnoi Probleme "Istoriia Velikoi Oktiabr'skoi Sotsialisticheskoi Revoliutsii," Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1973. Pp. 230.

ROGER PETHYBRIDGE. *The Social Prelude to Stalinism*. New York: St. Martin's Press. 1974. Pp. 343. \$18.95.

Even half a century later the Kronstadt revolt of March 1921 is bound to be a politically sensitive topic for any Soviet historian; Semanov's account does not even pretend to strive for objectivity. How was it possible that Soviet sailors and workers revolted against Bolshevik rule? The author must find exceptional circumstances to account for it:

exhaustion from war and privation, the severe food shortage, and, above all, the changing composition of the armed forces as a result of the influx of peasant and *declassé* elements. Semanov cannot indicate that this was Trotsky's explanation, too; he is obliged to assert that the rebels, mostly anarchists and socialists, were doing the dirty work for the imperialists. His own evidence fails to bear this out. He is at pains to show that the basic decision to embark on the New Economic Policy was taken before, and not because of, the uprising. As for the near famine, he manages to put the blame on Trotsky. He also accuses Zinoviev of losing touch with the working class in Petrograd and "in effect" leading a *fronde* against Lenin. Semanov must minimize the political objectives of the rebels; their opposition to the Bolshevik monopoly of power and their demands for civil rights are dismissed as petit bourgeois nonsense. A Soviet reader will scarcely be able to form an adequate idea of who the rebels were and what they wanted. Western readers will still need to consult such studies as Paul Avrich's *Kronstadt 1921* (1970).

The Social Prelude to Stalinism is a very different kind of book. It reflects the recent interest in the 1920s, years of relative pluralism on which fairly ample documentation exists. It also exemplifies the welcome recent growth of interest in Russian social history. The book is likely to be influential because of the questions it raises rather than the answers it provides. Professor Pethybridge, of the University College of Swansea, seeks to assess the role of social forces and conditions—as distinct from political and economic variables or the personality of the leader—in the mix of factors that produced Stalinism. In exploring the topic, he offers a number of astute observations and suggestive discussions on a wide range of issues—such as the impact of the civil war, the constraints imposed by illiteracy and poverty, the peasant nature of Russian society, family and population policy, and problems of nationalities, bureaucracy, and education. He is good on the confrontation of the early utopias with the dismal realities of post-1917 Soviet Russia. But in the end it is not clear just what, according to him, the total weight of social conditions was. While some of his comments on totalitarianism go in the opposite direction, the thrust of his argument—despite his disavowals—implies a bothersome element of determinism in the rise of Stalinism. Moreover, the entire treatment is neither concise nor exhaustive. If analytically unsatisfactory and perhaps too sweepingly negative in its assessment of the 1920s, this is nonetheless an interesting and challenging volume.

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RONALD HINGLEY: *Joseph Stalin: Man and Legend*. New York: McGraw-Hill Book Company. 1974. Pp. xxi, 482. \$15.00.

One of the most perplexing questions of USSR history, Stalin's rise to power with all its accompanying outcomes and disasters, still attracts the student of Soviet affairs. During 1973 two Stalin biographies were added to an already long list: Robert C. Tucker's *Stalin as Revolutionary* and Adam B. Ulam's *Stalin: The Man and His Era*; now comes the publication of an extensive, yet non-repetitious, version by Ronald Hingley, a noted British scholar. All three authors, while approaching their tasks from different levels and intentions, join to put to rest forever the legend of "good old Joe," as promulgated by President Franklin D. Roosevelt and other political figures during World War II. Instead the real, or almost true, Stalin emerges from their research.

In this process of deiconizing the tyrant, Hingley's work offers a magnificent exercise in demolishing the legend built for so long and intensively by Stalin and his henchmen. It is also a compelling study in how myths and legends emerge and how they can destroy the real person and necessitate a complete rewriting of history. For such is the case with Stalin who succeeded in creating an illusory image of benevolence while freely employing terror, destruction, and falsification during his three decades of unlimited power. To expose this deception and to penetrate the labyrinth of distortions became Hingley's herculean assignment. The search for the origin of the myth begins with Stalin's childhood and continues to the circumstances of his death in 1953. To this end the author has examined some four hundred books and sources, including the memoirs of former Stalin associates, using the method of direct confrontation between opposing views and revelations. Having established the degree of reliability of a given material, Hingley attempts to restore the most supportable course of events, with the result that in some specific cases he prefers to abstain from a conclusive opinion. For instance, the question of Stalin's collaboration with *Okhrana*, his responsibility for the massacre of the Tiflis May Day in 1901, and his disobeying Lenin's orders before and during 1917 are offered in a debating and not in a dogmatic style, despite obvious discrediting evidence. The confrontation between the legend and reality culminates in the dismembering of Stalin's role as a "war hero" during the Nazi-Soviet war. Hingley exposes not only Stalin's already well known strategic blunders but also such details as his only visit to the distant front line, contrasting it with the widely propagandized K. Finogenov drawing showing Stalin as a colossus "at the front."

This essay is both stylistically superb and a credible, scholarly contribution to its field. It offers a continuous challenge even to the knowledgeable reader, testing his own comprehension of the material and the ability to make references to other biographies. Furthermore, the impressive list of sources used is welcome and appreciated by the scholar who otherwise would have missed some of them. As to the author's specific suggestions, one has the freedom to accept or reject; however, the book remains a serious study. This is true despite some minor errors such as the author's confusion with the origin of Molotov's pseudonym, Skriabin. It does not derive from *mallet* but *molot*. Fifty-three illustrations, many of them very rare, optically underscore the conflict between the legend and reality. Instructors and graduate students in seminars on Soviet affairs are urged to submit this study to close scrutiny in order to test how legend is made and laid to rest.

STEPHAN M. HORAK
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Istoriia mist i stil Ukraïns'koï RSR [History of the Cities and Villages of the Ukrainian SSR]. *Kher'sons'ka Oblast'* [Kherson Province], edited by O. E. KAS'IANENKO *et al.*; *Kirovohrads'ka Oblast'* [Kirovograd Province], edited by D. S. SYVOLAP *et al.*; *Rovens'ka Oblast'* [Rovno Province], edited by A. V. MIALOVYTS'KYI *et al.*; *Sum's'ka Oblast'* [Sumy Province], edited by I. IA. MAKUKHIN *et al.*; *Ternopil's'ka Oblast'* [Ternopol Province], edited by S. P. NECHAI *et al.*; *Zhytomyrs'ka Oblast'* [Zhitomir Province], edited by O. S. CHORNOBRYVTSEVA *et al.*; *Kryms'ka Oblast'* [Crimea Province], edited by L. C. SOLODOVNYK *et al.* (Holovna Redaktsiia Ukraïns'koï Radians'koï Entsyklopedii Akademii Nauk URSR.) Kiev: Institut Istorii Akademii Nauk URSR. 1972; 1972; 1973; 1973; 1973; 1973; 1974. Pp. 686; 815; 654; 693; 639; 725; 801.

Six of these seven volumes closely resemble the preceding nineteen volumes of the monumental Ukrainian local history. The treatment of Ternopol province contains the same fascinating details about organization of Ukrainian nationalist ("Banderist") guerrilla activity as did the Ivan Franko volume. Numbers, positions, and sometimes names of Soviet officials killed by the insurgents while carrying out agricultural collectivization and suppression of "bourgeois" cultural and religious organizations are given for nearly every district, often for individual towns and villages. Such information for Rovno province—an Eastern Orthodox rather than a Uniate Catholic region—where nationalist guerrillas were active during World War II rather than in the late 1940s, is sparser. On the other hand, the volume on Rovno province, like those on other northern prov-

inces (Zhitomir and Sumy among those reviewed here), contains much valuable detail on early history, along with some delightful photographs of ecclesiastical and secular monuments still extant. Treatments of the southern steppe provinces, Kirovograd and Kherson, obviously cannot present a comparable wealth of early historical detail, although they do contain some useful points on local campaigns among Poles, Russians, and Tatars. Probably more valuable is the information on origins of settlers in this "New Russia" at the end of the eighteenth and during the early nineteenth centuries.

The last volume of the series on Crimea province is rather different from its predecessors. As I noted in an earlier review, the order of publication appears to have been due simply to the availability of professional historians. No doubt complications connected with the founding of Simferopol University (the first in the Crimea) during these years, together with the inherent complexity of Crimean history, delayed this volume. It is a striking coincidence, nevertheless, that the Crimea volume contains by far the most revealing material on nationality questions. The Crimea is, of course, a real ethnic palimpsest. I am quite unable to judge whether the discussions of archeological finds relating to the Scythians and earlier groups are of any interest to the specialist. It does not seem to me that the treatment of the Greek and Genoese colonies is particularly useful, although some details on the migration of remaining Greeks to the north shore of the Sea of Azov in the late eighteenth century and the photographs of surviving Greek buildings are interesting.

The discussion of the Tatars, on the other hand, is highly significant. The Crimea volume, like several others, emphasizes the centuries-old, common struggle of the Ukrainians and the Russians against the atrocious attacks of Crimean Tatars and their Turkish overlords. Evidently the editors considered stimulation of East Slav solidarity more important than possible offense to Turkic sensibilities. The editors' option for East Slav patriotism is still more evident in the treatment of World War II. Apparently the recent memory of Moslem Tatar "treachery" to the Soviet regime has become assimilated to the atavistic East Slav dread of depredations by the steppe nomads. The Tatars collaborated with the German forces in intense antiguerrilla activity in the Yaila Mountains and their foothills. Since the editors understandably take pride in the truly determined efforts of local Communists to carry on partisan activities in these regions—the only Crimean areas at all suitable for guerrilla warfare—the resentment expressed toward the Tatars is undoubtedly genuine. The final tragic act of the Tatars' wartime

resistance to Soviet rule, their expulsion from the land they had inhabited for seven centuries, is mentioned obscurely in a footnote on page 69: "In the spring of 1944, after the liberation of the Crimea from the Fascist occupiers, under circumstances of a continuing war, the Tatars who had lived in the Crimea were settled in other regions of the country. The facts of active collaboration with the German occupiers by certain portions of the Tatars were unjustifiedly ascribed to the entire Tatar population of the Crimea." The note assures the reader that (since a Supreme Soviet Presidium decree of 1967!) Crimean Tatars have enjoyed all their nationality rights—but in Uzbekistan and other union republics, rather than in the Crimea.

More attention is devoted to the recent settlement of the northern Crimean steppe by Slavs, and to the new Slavic settlements in the mountains and the southern coastal strip. The only clue to the existence of numerous Jews in the latter region is—as has been the case in treatment of the northern Ukraine—reference to extraordinarily high proportions of the "peaceful population" murdered by the Nazis. All in all, this volume is a historiographical counterpart of the symbol of Russian-Ukrainian partnership, which the Crimea became in 1954 when Khrushchev had the province officially transferred to the Ukrainian Republic. That being the case, it is hardly surprising that, despite vicissitudes in the Ukrainian Communist party leadership, the *History of the Cities and Villages* was allowed to proceed to completion.

JOHN A. ARMSTRONG
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NEAR EAST

ELIAS SHOUEFANI. *Al-Riddah and the Muslim Conquest of Arabia*. (The Arab Institute for Research and Publishing.) [Toronto:] University of Toronto Press. 1973. Pp. 180. \$10.00.

This monograph represents the kind of careful analysis Islamic historical studies need. The relevant materials used inevitably must be those already known. The author's thesis extends W. Montgomery Watt's work on Mohammed's relations with the Bedouin tribesmen to the period of the first caliph Abu Bakr; it argues the necessity of looking at the specific relationships each tribal group entered into with Mohammed, for their actions upon the death of the Prophet followed from these. That Bedouin tribes did not fall into simple apostasy, throwing off an accepted Islam, has been known for years; what the author here does is to sort out clearly just what actions were taken by each group and why. He shows that Riddah,

apostasy or falling away, can be properly applied to only a very few tribal groups; what, in fact, happened during Abu Bakr's time was Muslim conquest, or at least extension of power over Arabia and into the border areas. The book also re-emphasizes the importance of Meccan leadership, showing Mohammed's interest in wooing the Meccans and his policies favoring them. The author discusses Mohammed's evident interest in the lands north of the Hijaz toward Syria. The campaign of Tabuk is to be so seen, and although that came to little, events following it appear of capital importance for future policy: the imposition of obligatory *sadaqah*, or alms tax payments, and the forcing of idolaters with whom Mohammed had had treaty relations to become Muslims and pay the tax, or to fight him. Abu Bakr inherited these interests and pursued them.

More remarkable than the author's general analysis and conclusions is the careful way he handles his sources, teasing out important information through a careful sifting and criticism of individual accounts and Hadith reports. For example, he shows that a footnote listing Tabari is insufficient as a citation; it is necessary to state which account Tabari is himself using and what weight can be assigned it. In the work under review that method is not always followed; but the author expresses enough concern, particularly in pointing out problems using the reports by Sayf ibn Umar quoted by Tabari, that specialist readers and students of history generally are put on notice to be more careful using Tabari and the other standard sources. The real value of this monograph, therefore, is twofold: it brings into clear focus the tribal relationships with Mohammed and Abu Bakr, and it suggests how historians must handle the available sources.

A few minor problems in the book are the author's unacknowledged but clearly Sunni viewpoint of events on the day of Mohammed's death; several typographical errors particularly frequent in place names spelled with "H"; and the arrangement of the bibliography, where Julius Wellhausen's *Arabische Reich* (1902), in its Arabic translation (1958), is listed among Arabic works.

REUBEN W. SMITH
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W. MONTGOMERY WATT. *The Majesty that Was Islam: The Islamic World, 661-1100*. (Great Civilizations Series.) New York: Praeger Publishers, 1974. Pp. 276. \$20.00.

W. Montgomery Watt is a leading scholar of Islamic history and civilization and has been so for many years. He is the author of several books and articles dealing with the life of the Prophet Mohammed and also with Islam as a religion and way

of life. In this book, however, the author is mainly concerned with the rise and decline of the Islamic Empire between A.D. 661 and A.D. 1100.

The book is divided into five sections: "The Umayyad Period 661-750," "The First 'Abbāsid Century 750-850," "The 'Abbāsid Decline 850-945," "The Buwayhid Period 945-1055," and "The Earlier Seljūq Period." In each of these sections, the author first presents a narrative account of the history of the respective period. He then discusses somewhat briefly some religious and cultural aspects pertaining to that period, such as Islamic factionalism, jurisprudence, philosophy, theology, grammar and literature, and relations between Muslims and non-Muslims.

The book is well organized, well written, and very enjoyable to read. The historical narrative is quite accurate but very concise. The author's remarks about the various features of Islamic culture are very useful but also very brief. The reader is left with a strong impression of the author's great care for accuracy and fairness. In certain instances, however, the reader is left wishing that the author had elaborated more. For example, the reasons for the 'Abbāsid decline are discussed in fewer than three pages (pp. 159-61), and the replacement of Christian culture by Islamic culture is presented in fewer than four pages (pp. 257-60). Moreover, the author mentions that the transformation of the Christian culture in the Middle East into an Islamic culture is "one of the great failures of Christianity" (p. 2), but the reader is left wondering as to what aspect of Christianity is to blame for this failure. Perhaps a future work by the author will address itself directly to this very important question.

There is no doubt that by writing this book, Watt has once more put many students and scholars of Islamic history and civilization in his debt.

WILSON B. BISHAI
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AFRICA

J. D. CLARK. *Kalambo Falls Prehistoric Site*. Volume 1, *The Geology, Palaeoecology and Detailed Stratigraphy of the Excavations*; volume 2, *The Later Prehistoric Cultures*. New York: Cambridge University Press, 1969; 1974. Pp. xvi, 253; xiii, 420. \$14.50; \$49.50.

These are the first two volumes of a three-volume report on excavations undertaken at a number of locations around Kalambo Falls on the boundary of Tanzania and Zambia, close to the southeast corner of Lake Tanganyika. The site produced one of the longest archeological sequences so far found in sub-Saharan Africa, from late Acheulian to the present millennium—some sixty thousand years.

It was investigated between 1956 and 1966 by a team of specialists under the leadership of Dr. J. D. Clark, and the resulting report draws together all kinds of data that can throw light on the region in prehistoric times, in terms both of the influence the environment had on man in the area and also of the influence man exercised on his environment.

Volume 1 comprises a general description of the prehistory of the area as an introduction to the detailed discussion of the geology and paleoecology of the site with descriptions of the excavations. The second volume describes and analyzes the archeological data and interprets them in terms of human activity within the environment of the period, proceeding backward from an account of the contemporary ethnography of the area, through the Iron Age remains and the microlithic industries to the Polungu Industry of the closing stages of the Pleistocene about twenty thousand years ago. The third volume will take the story back to the late Acheulian and will also discuss the significance of the site and its place in the prehistory of the continent. This last section will perhaps be the one most interesting to historians.

The reversed chronological procedure may seem confusing to some readers, but not only does it represent the way in which an excavation recovers data; more important, it proceeds from the known to the unknown, from the way of life of the peoples of the present day and recent past to that of their predecessors in the area. Since the archeologist depends very largely on ethnographic analogy in interpreting his data this is a very effective and convincing approach. Moreover there is a very great emphasis throughout the book on consistency of terminology: this is, I think, the first major report to apply the terminology recommended by a Wenner-Gren symposium in 1965. The deliberate way in which terms are defined and applied has given a remarkable clarity to the whole book. In particular, the careful distinction that is always rigorously kept in the interpretive sections between observed archeological fact and ethnographic analogy brings out in a strikingly new way how dependent archeologists are on ethnography. One notices, too, that Clark wisely sticks close to the Kalambo Falls area in seeking his analogies, though occasionally he draws on descriptions of stoneworking techniques from further afield, usually checking them experimentally and comparing his own efforts with the archeological artifacts.

Comparisons are made with material from other excavations, but these are sometimes limited by the difference in methods of classification of artifacts. The need for a standardized terminology is apparent, but it is difficult to get some workers to

adopt one. This book will go a long way to encourage them. I have one misgiving, however, in that the term "component" is defined (p. 73) in a very different manner from the archeological usage of the word in the rest of the world. It would have been better to employ a term not previously used.

The techniques of analysis and the definitions of terms are exemplary. This work is destined to become a classic in archeological methodology.

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KENNETH POST and MICHAEL VICKERS. *Structure and Conflict in Nigeria 1960-1966*. [Madison:] University of Wisconsin Press. 1973. Pp. vii, 248. (Price not set.)

The overthrow of the first Nigerian Republic and the outbreak of civil war in 1966 signaled the collapse of West Africa's most important experiment in the application of the Westminster model of parliamentary democracy. Four years before the coup, Henry L. Bretton argued in *Power and Stability in Nigeria* that Nigeria's legal-constitutional system rested on quicksand and might itself become a major source of instability. Yet to most observers, the ease with which Major Nzeogwu and his military associates overthrew the republic came as a complete surprise.

In this useful study of the six years just before the Nigerian coup, Kenneth Post and Michael Vickers offer an analysis of the political and social system of the First Republic based on their interpretation of events centering around the federal elections of 1964-65. The disastrous attempt to elect a new federal legislature should not be seen, they argue, as an isolated failure, but as the capstone of a prolonged crisis of the political system of Nigeria. Post and Vickers attribute the progressive decay of the precoup political system to several basic factors: the tenacity and strength of entrenched ethnic groups, each commanding a transcendent loyalty in its home region as well as among federal politicians and civil servants; colonialism, which differentially incorporated ethnic groups into a common political system and forced them to battle each other for political and economic rewards; the greed of the politicians, who plunged Nigeria into chaos by seeking to extend their dominion beyond the boundaries of their home regions; and the constitutional-political structure of the Nigerian Republic, which, as Bretton had predicted, fatally reinforced competition between regionally based ethnic groups.

Structure and Conflict is a "preliminary essay" that marks the beginning of serious efforts to achieve a fully integrated analysis of Nigerian society in the 1960s. The results of the authors' grueling forays

into building models of the political superstructure rarely go beyond the obvious and fall short of their goal, but in the process these two political scientists tell historians a great deal about the development of the Nigerian crisis and offer a number of fruitful suggestions about the interaction of culture and political structure. Post and Vickers have written a serious, provocative, and badly needed study. They have raised many more questions than they have answered, but that is in the nature of the exercise.

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ASIA AND THE EAST

FRANK A. KIEMAN, JR., and JOHN K. FAIRBANK, editors. *Chinese Ways in Warfare*. (Harvard East Asian Series 74.) Cambridge, Mass.: Harvard University Press, 1974. Pp. xii, 401. \$18.50.

The introductory essay and seven articles contained in this volume may be considered from the perspective of three crude generalizations about Chinese militarism and military institutions during the Han dynasty and from the middle of the eighth until the nineteenth century. The first is the association of military heroism with the role of emperor. Dynasties were founded and legitimized by conquest, rival centers of power were destroyed or subjugated, and control over the frontiers was re-established. The imperial heirs owed their position to the founder's military prowess and, irrespective of their talents, temperament, or circumstances, were constrained to honor it, if only by ceremonial means within the official cult. The second generalization is that in a predominantly labor-intensive agricultural economy, most military and civil officials were, or became, men of property with a strong interest in internal peace and order. War reduced production and invited loss and destruction of wealth. This, added to the fact that the inner Asian frontiers were mostly poor and not suited to large-scale Chinese settlement, discouraged adventurism on the part of officials. The third is to the extent that Chinese imperial government became depersonalized and bureaucratic, it was inimical to the heroic style. Martial values had their deepest roots in the picaresque counter-culture of smugglers, bandits, rebels, and hired bravos who had much to gain and little to lose by violence, and they were widely celebrated in popular drama, fiction, and temple iconography.

John Fairbank's essay, "Varieties of the Chinese Military Experience," sets the military aspect of government in the context of an empire politically dominated by a civil bureaucracy. Military tasks

sometimes had to be taken up by civil officials, but were often left in the hands of non-Chinese militarists of the frontier regions. The author offers the hypothesis that the Mongol and Manchu conquests can be regarded as an arrangement welcomed by "a sufficient number of Chinese" whereby the tasks of unifying and defending the empire were turned over to alien groups who had the ethnic solidarity and military orientation to perform these services successfully.

Frank Kierman's "Phases and Modes of Combat in Early China" contrasts the religious and ceremonial character of warfare in the seventh century B.C. with what he calls the "rationalization of warfare" by the third century B.C. Michael Loewe's "The Campaigns of Han Wu-ti" uses the inner Asian expeditions of 129-90 B.C. to demonstrate the severe logistic, geographic, and political limitations on Chinese imperial expansion even at this time when Han military power was at its height.

Charles A. Peterson's "Regional Defense against the Central Power: The Huai-hsi Campaign, 815-817," Herbert Franke's "Siege and Defense of Towns in Medieval China," and Edward L. Dreyer's "The Poyang Campaign, 1363" give this volume a solid core by their detailed examination of internal warfare within the Chinese Empire, and by their chronological continuity. All contribute to an understanding of the military and political significance of the fortified Chinese city. Franke's article is especially valuable for his careful attention to the relationship of urban defense to medieval class structure. Dreyer clarifies the significance of inland naval warfare in the fourteenth-century civil wars.

Frederick W. Mote's "The T'u-mu Incident of 1449" and Charles O. Hucker's "Hu Tsung-hsien's Campaign against Hsu Hai, 1556" shift the focus to the northern frontier and to coastal defense respectively. The capture of Emperor Ying-tsung is employed to illustrate a persistent misunderstanding of the strategic problem of the northern frontier. This led to a commitment to sterile and inappropriate military solutions until the end of the Ming. Hucker has provided a contrasting study of the astonishingly imaginative array of means, including trickery and deceit, employed by a civil official charged with pacifying an east coastal region ravaged by island-based marauders.

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ELLSWORTH C. CARLSON. *The Foochow Missionaries, 1847-1880*. (Harvard East Asian Monographs 51.) Cambridge, Mass.: East Asian Research Center,

Harvard University; distrib. by Harvard University Press, Cambridge, Mass. 1974. Pp. 259. \$8.50.

The author, who spent four years teaching in China, examines the work of American and British missionaries in Foochow during the years 1847–80. His primary sources are largely letters and reports of missionaries; he makes limited use of available Chinese materials. The major emphases are on the aims of the missionaries, on the methods they employed in efforts to win converts, their attitudes toward the Chinese, and the reasons for and the extent of Chinese opposition and the forms of the opposition. The analysis of each is marked by close attention to accuracy, avoidance of over-interpretation, and carefully balanced judgment of the protagonists. From a scholarly viewpoint the volume is a significant compilation of factual evidence concerning the missionaries in one Chinese city. Carlson is frank in his portrayal of individual missionaries who manifested attitudes of arrogance, self-righteousness, and unwillingness to adjust to Chinese feelings and traditions, but he also tries to understand them and to put before the reader the frustrations they experienced. The account does not differ in interpretation from those presented by other scholars in recent years.

The most valuable contribution is the author's detailed treatment of Chinese efforts to curtail the missionaries' efforts and the motivation of the Chinese in placing obstacles in the way. The overwhelming and deep-seated hostility throughout the period had its source in antiforeignism, in the belief of the gentry that Christianity endangered existing institutions and portended revolution, and the sheer persistence of the missionaries in demanding their rights and more under the treaties. The ingenuity of the Chinese in curtailing treaty rights in the face of threats of the use of force and the stolid, ethnocentric digging in in the face of violence and persecution by the missionaries guaranteed day-to-day confrontation and riots. The author's understanding of the Chinese side and his scaling down the self-righteous indignation of the Westerner make for exciting reading.

PAUL A. VARG
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J. D. FRODSHAM, translated and annotated by. *The First Chinese Embassy to the West: The Journals of Kuo Sung-t'ao, Liu Hsi-hung, and Chang Te-yi*. New York: Oxford University Press. 1974. Pp. lxxv, 222. \$13.00.

Here is a book that deserves a very wide audience. Although it may appear to be too specialized to interest many readers apart from historians of China, it should be highly rewarding to non-specialists. J. D. Frodsham has translated the journals of Kuo Sung-t'ao, China's first ambassador to

the West, and parts of the journals of Kuo's assistant, Liu Hsi-hung, and their interpreter, Chang Te-yi; he has also provided a selection of related items, mostly written by Kuo, as well as the diary kept by one of Kuo's two English secretaries, Dr. Halliday Macartney, and an excellent introduction that places the mission in its historical setting and comments perceptively on the writers and their differing views of their common experience. The translations are painstakingly annotated and, insofar as I can tell from some random comparisons with the Chinese texts, quite accurate. The entire book reads smoothly.

China specialists will not find here anything that is momentarily new, but Frodsham has fresh perspectives, and the texts offer a wealth of enlightening detail about the nineteenth-century Chinese-Western encounter. Frodsham observes that Kuo was naive: "Kuo's work was in essence but the record of his ignorance of the West. [Kuo himself once asked: 'How can I, who know no foreign language and am ignorant of world affairs, fill my post competently?'] Yet it is precisely in this ignorance, in the freshness of response that it evokes, that much of the charm of his work lies." There is indeed charm in the work, even and perhaps especially in the abundance of trivia. There is also substance. Kuo was an experienced, curious, thoughtful man. He had keen insight and an open mind. An outspoken reformer at a time when such views were dangerously unpopular, Kuo was by no means an uncritical admirer of the West. But when he had favorable comments to make about Western strengths he made them bluntly. To his colleague, Liu Hsi-hung, however, Western learning was mere technology, "exotic arts," and "petty, miscellaneous tricks" that produced "novelties"; later, when Liu saw for the first time in his life trains and other modern wonders, he concluded, "This is certainly a clever construction, but far from indispensable." These views notwithstanding, Liu found things to admire in Europe, although he still insisted that the West had little or nothing to teach China.

The contrasting views and personalities of the Chinese observers, their interaction with each other and with the British, and Europeans' reactions to them make up a lively and fascinating chapter in world history. Frodsham has performed a service that enlightens and delights.

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DAVID L. DAVIS *et al.* *Medieval Japan: Essays in Institutional History*. Edited by JOHN W. HALL and JEFFREY P. MASS. New Haven: Yale University Press. 1974. Pp. xv, 269. \$12.50.

Seven of the eight contributors to this volume are either Ph.D. candidates or recent recipients of the degree. If the volume is a fair sample of their work, the study of medieval Japanese history clearly is moving into a take-off stage. There is not an essay in the book that does not enlarge our understanding of medieval Japan, either by providing new historical information or by suggesting new lines of interpretation.

As the subtitle indicates, the main focus of the essays is the development of political institutions from the reception of T'ang civilization to the emergence of the Muromachi *Bakufu*. Either directly or indirectly, all the essays explore the *morcellement de pouvoir* that resulted from competition between capital and province, court and aristocracy, and metropolitan elite and regional warriors. John W. Hall establishes this theme in the opening essay, which describes how changes in the physical configuration of the capital city reflected the shift of power relationships among the imperial family, the civil aristocracy, and the provincial warrior elite. It is the least technical of all the essays and doubtless will be of value to specialists in fields such as art history as well as to historians.

In the next two essays G. Cameron Hurst illuminates the character of Heian court politics through a study of aristocratic household governments, patterns of aristocratic patronage, and the role of the abdicated emperor's court. He suggests that the four centuries of aristocratic ascendancy were not a period of decline, and that the Heian courtiers developed highly sophisticated institutions to compete for preferment and land rights, the dynamic forces behind Heian politics. Elizabeth Sato's essay on the development of the *shōen* confirms this. Her essay also suggests that the accumulation of rights in private estates was a far more complex process than earlier accounts (including that of Asakawa Kan'ichi) have suggested.

In a diffuse but provocative essay, Cornelius J. Kiley sets forth an equilibrium model of late Heian politics. He argues that the resilience of aristocratic government rested on the community of interest the *shōen* system established among its participants at all levels. He suggests that the *shōen* was not an institution that strengthened local interests at the expense of aristocratic, but that it was a corporate "estate community" controlled by "segmental factions" including both aristocratic patrons and local landed elements. To the extent that political conflict occurred, it centered around factional disputes, not class conflict or capital-provincial rivalry. In a brilliant insight, he also suggests that the very looseness of the political structure—"extreme tolerance for autonomous groups"—helped to inoculate the system against the peri-

odic upheavals that plagued the Chinese imperial structure.

In two essays on the Kamakura period, Jeffrey Mass gently challenges the views of his coeditor and *sensei* that there were important continuities between Heian and Kamakura institutions. Relying on a careful study of documentary sources, he concludes that Yoritomo established his regime in Kamakura in a deliberate effort to break away from the authority of the court, if not to supplant it. His methodology suggests the danger of relying too heavily on later literary accounts and chronicles. Kyotsu Hori's essay on the impact of the Mongol invasion argues, as others have, that the invasion was important in weakening the Kamakura regime, but suggests that it had this effect mainly because it accelerated long-term tendencies of the institutional structure, such as the collapse of the *sōryō* system, the shift to impartible inheritance, and the growing gap between the dynastic interests of the Hōjō and the local interests of provincial warriors.

The two essays by Prescott B. Wintersteen, Jr. show how the Muromachi *Bakufu* displaced the court and aristocracy as the arbiters of capital politics and how in turn its own authority in the provinces was eroded by the granting of emergency financial powers to the *shugo*. David L. Davis also illuminates an important aspect of the Muromachi period by describing for the first time in English the institution of the *ikki*, which bound samurai, and also commoners, by peer loyalty and territorial proximity rather than by kinship, aristocratic patronage, or vassalage ties. He suggests, not very convincingly, that in this new organizational principle lay the sprouts of modernization.

Nearly all the essays raise issues and define problems within the boundaries of the scholarly discourse established by Japanese historiography. Herein lies the volume's strength as well as its weakness. It gives us a better picture of "what really happened," but fails to raise broader issues such as the nature of Japanese feudalism. Only the essay by Cornelius Kiley, informed by his acquaintance with political anthropology, suggests tantalizing possibilities for further comparative study.

Let me add two minor cavils: a number of key terms (e.g., *ko* on page 81 or *koei* on page 121) are not to be found in the otherwise valuable glossary, and there are no biographical notes on the contributors, who deserve to be identified more completely.

PETER DUUS
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PHILIP MASON. *A Matter of Honour*. New York: Holt, Rinehart and Winston. 1974. Pp. 380. \$12.95.

The author, already distinguished by a two-volume account of the administrative and social background of the British in India, has placed us further in his debt with this account of the Anglo-Indian armies from their beginnings in the eighteenth century to 1947, the year the Indian subcontinent formally regained its independence from Britain.

It is based wholly upon published works and is lit with insights gained from the author's twenty years of administrative service in India. While one may regret that he did not use some recently published works—for example, J. A. Norris's *The First Afghan War 1838-42* (1967)—or the untapped riches in periodicals such as Colburn's *United Service Journal* (1829-1920), the core of the book remains unflawed.

It is not military history in the commonly accepted sense of battles and marches. Instead it attempts to answer questions that have significance beyond British India. What inspired Indians to fight so well in causes not their own, for men whose looks and customs were different, and whose behavior was often both incomprehensible and offensive? How did these Anglo-Indian armies so often beat larger armies—armies more often than not trained and led by European professionals? Why did these Indian regiments remain loyal when it was far from their interest to do so? What inspired the handful of mutinies that shadow those two hundred years? How, after the great mutiny of the Bengal Army in 1857, did a new army emerge from the ashes, and in what way did it differ from the old? What did these armies borrow from the past, from their enemies, from the British? What did officers and men think of each other, and in what ways did their relationships change over time? How did the armies fit into the structure of the empire they had helped win?

Roughly four-fifths of the book is devoted to the years before 1914. If for the years following the author displays less sureness of touch—a fact revealed by his increasing dependence upon campaign history—he is in good company, for we are all feeling our way among the rubble of this century. If, years from now, a historian can bring to the last generation of the Anglo-Indian army the same clarity, compassion, and understanding that Mr. Mason has brought to those generations before it, this will be an event well worth waiting for.

JOHN TOTTENHAM
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BAWA SATINDER SINGH. *The Jammu Fox: A Biography of Maharaja Gulab Singh of Kashmir, 1792-1857*. Carbondale: Southern Illinois University Press. 1974. Pp. xiv, 263. \$15.00.

Biographies, which constitute such a large segment of South Asian historiography, suffer from

an important didactic weakness: their preoccupation with the particular usually results in a weak analysis of larger but related issues. This biography of Gulab Singh is no exception. The author concentrates so intently on the mid-nineteenth-century dogra maharaja of Kashmir and Jammu that he ignores, no doubt deliberately, questions of greater historical significance.

The primary purpose of the book is to present a thoroughly researched and "dispassionate account" of Gulab Singh's career, particularly his role in the complicated events and intrigues that followed the death of Ranjit Singh and led to the destruction of the Sikh state. In this respect it is a fine example of biographical scholarship. It portrays Gulab Singh as a Machiavellian power broker and political opportunist who so successfully elevated survival above all other considerations that the British ultimately appointed him maharaja of Kashmir. Whether or not duplicity, cruelty, and avarice are admirable is not, however, a question in the book. Instead, it emphasizes that Gulab Singh, unlike his contemporaries in the Punjab, possessed both the ability and the foresight to make amorality work for him. This unemotional approach results in a balanced account of Gulab Singh's activities and as such broadens our knowledge of the political events of northwestern India during this period.

It is, then, an excellent biography. But, perhaps because it is so preoccupied with presenting a neutral account of Gulab Singh's career, the book stops short of any analysis of the structure of the Sikh state or the character of traditional political power in South Asia. A good example is the author's treatment of the question of communalism. The author, by virtue of research in hitherto unused private papers, is able to quote Governor-General Hardinge to the effect that the British made Gulab Singh maharaja of Kashmir because they preferred Hindu to Muslim rulers. Yet the statement is left unexplored. Similarly, the author's assertion that Gulab Singh was reluctant to employ Muslims in his administration in Kashmir and actually removed many is not pursued or related to a statement on the maharaja's religious tolerance made several pages later. The lack of any analysis of these points, however brief, is frustrating, especially in light of the importance of the Kashmir question since 1947. The fault here, however, is not in the scholarship but in the form; an unbiased narrative of one man's career is attained at the expense of analysis.

NEIL RABITOY
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NICHOLAS MANSERGH, editor-in-chief. E. W. R. LUMBY, assistant editor. *The Transfer of Power, 1942-7*. Vol-

ume 4. *The Bengal Famine and the New Viceroyalty, 15 June 1943–31 August 1944*. (Constitutional Relations between Britain and India.) London: H. M. Stationery Office; distrib. by Pendragon House, Palo Alto, Calif. 1973. Pp. xcix, 1295. \$52.00.

This important collection of source materials must surely serve as required reading for any serious historical interest in twentieth-century India. The latest volume of documents covers the final weeks in the viceroyalty of Lord Linlithgow (April 1936 to October 1943) and the first months in the regime of Lord Wavell. It represents the official record of momentous events that transpired within fourteen months during the last half decade of British rule. It provides a fascinating glimpse of the twilight of the raj, of both rulers and ruled, as seen from the perspective of New Delhi. In the midst of a vast global war, struggles no less fateful for peoples of the Indian subcontinent were reaching points from which there could be no return. Within one year (from August 1943 to August 1944) profound forces of change seem to have been let loose. Indian troops went off to battle in North Africa and Burma. Domestic tranquillity, irreparably shattered by the triple hammerblows of the Lahore Resolution, the Cripps Mission, and the Quit India Rebellion, was never again to be the same. And immediately thereafter came the great famine of Bengal. It wreaked unprecedented carnage upon those most wretched and poorest of peoples—that half of the population which ever had clung precariously to life.

If any single theme seems to emerge from these papers, like a recitative in variations, it is sense of loss—deep disappointments, dashed dreams, broken faiths, and breached relationships on every side. The last hopes for preserving a united India within a federation of a self-governing and completely independent dominion seemed to fade. By words and deeds, Congress leaders had resisted the India Act of 1935, had set themselves against provincial autonomy in 1937, had discouraged princes from constructive participation in federating schemes, had resigned from positions of power and responsibility in protest against the war, and finally had made a futile effort to drive the British out of India by force. Now they found themselves in jail for the duration of the war. Leaders of right-wing Conservatives in Britain, Churchill among them, had matched the Congress blow for blow in shortsighted and reckless policies. In consequence, while the princes had remained petrified, indecisive, and immobile, much of the remaining Muslim support for a united India had been drained away, and Muslim commitments to partition had grown ever more irrevocable. Only later, when already too late, did Gandhi begin to wish that the Congress had helped to bring the federation into being.

But it was Rajagopalachariar (in the newspaper *Amrita Bazar Patrika*) who saw most clearly. Perceiving the dangers, he lamented “the soul-killing negative attitude” of Congress leaders, their refusal “to correct past mistakes,” and “lack of foresight.” “Magnificent opportunities” had been lost. It was fallacious to view Indo-British affairs simply as “a struggle between arrogant Imperialism and uncompromising Nationalism. . . . The forces concerned [were] many, and the conflict many-sided. . . . British Imperial policies [had] always been a mixture of national self-interest and certain truly noble ideals, whereas Indian Nationalism [was] not the irresistible longing of a homogenous and united people ready to sacrifice their all in their thirst for freedom.” Muslims had set up a rival feeling with “widest appeal among the Muslim masses.” Industrialists were making “quiet and uninterrupted services at the call of a bureaucratic Government while shedding copious tears for Nationalism.” Vast problems of poverty and illiteracy were heavy drags, without quick or easy solutions. States and zamindars, and countless caste and sectarian jealousies, pulled people in many directions. “In this entanglement of forces, the possession of an unbroken tradition of loyal service [was] not a negligible factor. Unfortunately, however, Indian Nationalism [had] so long been in opposition that it [seemed] incapable of realizing that it [could] make honourable use of power and responsibility.” Nonviolent resistance had served as a method for agitation and for regenerating the spirit. “But as a positive means to bring about the transfer of political power to representatives of people composed as [India’s] it [had] proved inadequate. At the critical point where resistance should end, and persuasion and formative compromise be requisitioned so as to save and fix the moral gains, there [was] a gap, and all that [had] been achieved [could melt] into mere history. [India’s was] not a condition in which anarchy could automatically crystallize into ordered self-government.” For the sake of preserving past gains and of building a new union as an ultimate expression of genuine national integration, Rajagopalachariar pleaded with the Congress leadership to join the government.

But alas, Rajaji’s words were not heeded. Narrower interests were to prevail—on all sides.

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CHARLES A. JOINER. *The Politics of Massacre: Political Processes in South Vietnam*. Philadelphia: Temple University Press. 1974. Pp. xv, 346. \$12.50.

This is not one of those books on Vietnam that is going to make a great splash, despite the irrelevant

title. In fact, other than a lengthy chapter on the presidential election of 1967—well placed in historical perspective but superficial in political analysis—the book is essentially the reprinting of research previously published between 1963 and 1970, including three articles jointly authored. Nevertheless, it does constitute one of the fullest scholarly accounts available of the internal politics of South Vietnam in the late 1950s and 1960s. Giving prominent attention, quite appropriately, to the centrifugal forces in South Vietnam, Professor Joiner is most complete in his coverage of political parties, the Buddhists, and the montagnards. His treatment of the National Liberation Front (NLF) is simply a commentary on Douglas Pike. He also devotes considerable space to bureaucrats and the Vietnamese administrative system, understandable for someone who began his career in Vietnam as a staff member of the Michigan State University Advisory Group.

The book opens boldly with the claim, "This work is highly critical. . . . It is critical of American foreign policy . . . of the several governments of South Vietnam, . . . and of the government of North Vietnam." Though this is undoubtedly a sincere expression of Joiner's present sentiments, such views are unfortunately not nearly so prominent in the text as his preface would have us believe. In fact, the book can be contrasted with much of the literature on Vietnam because of its description of political events and institutions without assigning blame for the frequently tragic consequences of policy. Many scholars would applaud this approach; moral judgments should not clutter academic writing. But given the focus in this volume on politics within areas controlled by the Republic of Vietnam (Saigon), such an approach more often than not amounts to withholding criticism of U.S. intervention.

Nothing is said, for instance, of the American role in the overthrow of Diem, perhaps because the passage in question was written in 1964. Nor, in fact, is there a discussion in any chapter of the impact of the CIA on numerous occasions, even though this topic has loomed large in the political conversation of sophisticated Vietnamese for many years. No mention at all is made of the tremendous pressure from Lyndon Johnson on Saigon to hold a presidential election before the U.S. election in 1968, although most observers are agreed that such pressure was the primary stimulus for the Vietnamese polling in 1967. It is refreshing, however, for Joiner to make the point quite clearly in more than one passage that it was the massive American military presence from 1965 that led to the increased North Vietnamese participation in and direction of the Communist struggle in the South.

I am in sympathy with the author's position that

"it is the political processes of residents of South Vietnam . . . that explain the failures or successes of external forces and ideologies struggling on South Vietnam's soil" (p. xii). Yet an adequate analysis of those processes would hardly seem possible without clearly identifying the external forces. On the status of North Vietnamese in the South, Joiner seems to be ambivalent. At one point he refers to Tonkinese as "foreigners" (p. 296) and at another calls Vietnam a "divided nation" (p. 230). He calls the "question of whether Hanoi represents a foreign power infringing upon the rights of residents of South Vietnam" a "complex problem" and never answers it squarely (pp. 6-7). Nevertheless, at several places in the book the status of North Vietnamese as "outside intruders" is implicitly confirmed. On a related question Joiner finds the role of Hanoi in the formation of the NLF "shrouded in mystery" (p. 8). In any case he seems to place greater emphasis on the strength of Cochinese regionalism than does Pike, at this as well as at other turning points in Vietnamese history.

In sum, there is much in this volume that is valuable, in both description and interpretation. It suffers, however, from factual gaps and lack of coherence because of Joiner's failure to revise or update his earlier articles. And the mild, even-handed rebuke to all combatants is likely to satisfy very few readers. For it is hard now to conclude that all parties to the Vietnamese conflict have, in fact, carried equal moral responsibility for destruction of a society.

DAVID WURFEL

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SARTONO KARTODIRDJO. *Protest Movements in Rural Java: A Study of Agrarian Unrest in the Nineteenth and Early Twentieth Centuries*. (Issued under the auspices of the Institute of Southeast Asian Studies, Singapore.) New York: Oxford University Press, 1973. Pp. xv, 229. \$16.75.

This book by Indonesia's most eminent modern historian will be of great interest not only to specialists on that country but to students of peasant movements in general. Professor Sartono has the advantage not only of being thoroughly at home in the culture he is describing but of having a broad interest and solid grounding in sociology and anthropology as well as history, and he uses these tools to good effect in developing the multidimensional approach of his study.

In accordance with his conviction that an analytical framework would be more fruitful than a chronological one, Sartono has arranged his discussion according to what he sees as the primary characteristics of the movements. There are chap-

ters on antiextortion movements, messianic movements, revivalist and sectarian movements, and local Sarekat Islam movements—these last representing a blending of traditional and modern forms of organization and protest. Within these categories, he considers the movements in terms of their mass base, leadership, and ideologies, of the political-economic structure of rural Java, and of the “cultural conduciveness of the society in which these social movements are found” (p. 4).

Sartono's book thus shares with Eric Hobsbawm's *Primitive Rebels* (1959) an approach through categorization; it shares as well an evolutionist assumption of development toward greater consciousness and more modern and effective forms of organization. “What is important,” he tells us, “is that the fundamental tendency of the period after 1910 was the progressive adoption of modern traits” (p. 189). Maybe so, but this is scarcely proved by the examples he provides, which show “archaic” types of protest arising well after the florescence of the Sarekat Islam. Categorization of phenomena is a useful heuristic device for introducing a wide range of examples, but it is static, bears the danger that the category may become more important than the substance, and makes it difficult to develop successfully a discussion of the movements in their environment and their change over time.

Professor Sartono is surely justified in his hope that this book will reduce the Eurocentric tendency in the writing of Indonesian history. Certainly the rich variety of movements he discusses puts to shame those who have argued that there is little information on the doings of the rural population under colonialism. His own masterly, in-depth investigation of one such movement, *The Peasants' Revolt of Banten in 1888* (1966), has already indicated the wealth of archival material that may be found concerning a peasant outbreak; and the capsule histories of nearly two dozen movements that are given here should be a beacon for research for many years to come.

RUTH F. MCNEY

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SIONE LĀTŪKEFU. *Church and State in Tonga: The Wesleyan Methodist Missionaries and Political Development, 1822–1875*. Honolulu: University Press of Hawaii, 1974. Pp. xvii, 302. \$12.00.

It has generally been stressed by historians and other analysts of the polity in the Far Pacific that missionaries played the most important role in its development. Missionaries themselves are largely responsible for such a historiography; their claims to achievements of this kind were meant to justify

their work to financial supporters back home or to hold a better leverage with British officials in the islands, and to pat themselves on the back for bringing civilization, including governmental institutions, to distant heathens. The antimissionary tracts, fewer in number, have held a diametrically opposite view.

The volume under review is significant in many respects. It is one of the numerous publications resulting from ambitious programs launched by Australian and New Zealand universities to study the Pacific. It was first submitted as a Ph.D. thesis at the Australian National University. The author, Sione Lātūkefu, a scion of one of the prominent families of Tonga, an ordained Methodist minister, and a senior lecturer at the University of Papua, has worked closely with the missionaries and governmental archives in addition to using oral tradition collected from Tongans during a “field trip” to his native land. He had a unique opportunity to spend four weeks with the ailing queen of Tonga in Auckland, where the queen filled him with her “wealth of knowledge of the Tongan traditional past.” The study is fairly objective, relatively free from chauvinism, church pride, or an alienation that creeps into individuals working outside their land of birth. Finally, it is a work completed without having to visit England; all the documentation, some of it never before consulted, like the papers of Reverend R. G. Page, was available to Lātūkefu within the Pacific area.

The focus of the work is the period from the re-establishment of the Methodist missionaries in 1826 to the promulgation of constitutional monarchy in 1875. In Lātūkefu's view, the missionaries served to accelerate the evolution of a polity in that direction but did not motivate or even provide the prime power to the process. Through a careful analysis of the Tongan religious and secular authority, first combined in the Tu'i Tonga, bifurcated in the fifteenth century by creation of a new position of *hau* (temporal ruler), the author brings us to a crisis in the middle of the nineteenth century brought about by rival factions in the family of Kanokupolu, which had traditionally held the temporal authority. He outlines three different new codes of laws, the result of efforts of Tongan rulers in the nineteenth century on their own, without much missionary influence. The crisis of succession in the Kanokupolu family in the mid-nineteenth century was matched by a crisis of confidence in traditional Tongan deities. The blow to such divine authority was given by the conversion to Christianity of Taufa'ahau (later King George), who first tested the heathen gods and then the Christian god by a series of tests that included throwing his companions to sharks and retrieving them. The least convincing part of the

book is the background of this conversion, which Lātūkefu describes more in the style of a proselytizer than as a scholar analyzing the political reasons that certainly influenced it. In Dr. Lātūkefu's judgment, the missionary influence was important, but not decisive, in the political process leading to the Constitution of 1875. The argument thus strikes a middle ground between the promissionary and antimissionary writings on the role of the church in the development of Tongan polity in the last century.

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TOM GIBSON. *The Maori Wars: The British Army in New Zealand, 1840-1872*. (19th Century Military Campaigns.) London: Leo Cooper; distrib. by Shoe String Press, Hamden, Conn. 1974. Pp. 271. \$13.50.

Tom Gibson is a professional soldier and brings to this account a trained eye for the lay of the land, a sympathy for the conditions under which the rank and file fought, and an overall appreciation of the political and military problems faced by field commanders in New Zealand in the mid-nineteenth century. His account is a straightforward, chronological retelling of the well-known facts from the perspective of the British army and draws heavily on regimental histories and other published works for most of its material. The book is intended for the general reader rather than the scholar. The illustrations are excellent, but the campaign maps are difficult to follow, and the footnotes are virtually useless, since they give only author and title. The bibliography supplies some of the missing data, but the date of publication and publisher are not always listed.

Given the campaign focus of the book, one should not expect the author to probe deeply into the causes and consequences of the wars or to plumb the conflict from the viewpoint of either the Maori or the settler. Even the general reader, however, will be disappointed that Gibson provides little more than a blow-by-blow account of each skirmish, battle, and campaign. Twelve regiments of the line saw active service in New Zealand, together with a field battery, engineers, the military train, and the commissariat staff corps. These units come and go as if they existed in a vacuum. Gibson tells us almost nothing about the organization of the British army. He does not explain the structure of command, the kinds of men recruited or experience and skills they brought with them, how the army adapted itself to local conditions and attitudes, what the problems of communication were within the colony, with Australia, and

with Whitehall, and how the movement of troops and supplies, whether by land, sea, or river, affected the conduct and outcome of campaigns.

Indeed, the lesson of this book seems to be that military history is too important to be left to the professional soldier. Gibson's style lacks distinction. He assumes more background knowledge than most general readers will have and writes from the outmoded traditional perspective that once the army had crushed the "rebellious natives," *pakeha* and Maori were reconciled to each other and to racial brotherhood.

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UNITED STATES

FRANK FREIDEL, editor, with the assistance of RICHARD K. SHOWMAN. *Harvard Guide to American History*. In two volumes. Rev. ed.; Cambridge, Mass.: Belknap Press of Harvard University Press. 1974. Pp. xxx, 605; xxvi, 610-1290. \$45.00 the set.

JOHN A. GARRATY, editor; JEROME L. STERNSTEIN, associate editor. *Encyclopedia of American Biography*. New York: Harper and Row. 1974. Pp. xiv, 1241. \$22.50.

American historians have long awaited publication of this revision of the *Harvard Guide*, for the 1954 edition and its predecessors had proved invaluable and practically indispensable to all students of the subject. The revised edition fulfills our expectations and in many ways surpasses in usefulness the 1954 *Guide*. Most noticeable is the improved readability. Whereas the older version's chronological sections ran entries together in paragraph form, this edition begins each entry with a new line. For a basic reference tool such as this, easy readability is an immense virtue. This virtue, and the thirty-three per cent new entries, necessitated expansion of the *Guide* to two volumes, with an accompanying increase in cost. The improvements fully justify the added expense.

The major change in organization is the shift from one chronological list of books and articles to two types of lists. Volume 1 contains topical entries and sections on the nature of the profession, original sources, and finding aids; volume 2 encompasses chronological listings, the name index, and the subject index. Discussions of the profession, sources, and guides have been rearranged considerably, but the information remains essentially the same. While the editor obviously had to delete material from the 1954 book to incorporate recent scholarship and to reflect changing emphases in the profession, he also chose to remove summaries from the openings of bibliographical chapters, as

well as the sections on historical sources, on the grounds that "few readers now use them" (p. v). Such may be the case, but each section was valuable, especially that on sources. Unless researchers eschewing the latter had more comprehensive finding aids available, they ignored the material to the detriment of their research, for in historical writing mastery of primary sources remains essential. If only as a heuristic device, the citations of sources should have been maintained.

Responding to the trend of historical publications in the last two decades, the revised edition includes far more documentary histories and edited books of readings than its predecessor. Also, the new topical arrangement has enabled greater catholicity of selection, so that the *Guide* more accurately reflects the American past. For example, the section on sport history now includes such popular works as *Instant Replay* and *Ball Four*.

Although twenty years have elapsed since the initial publication of the *Guide*, this new edition does not account for that many years of publication, for the cutoff date for inclusion in the present volumes was June 30, 1970—a full four years before publication. Such a time lag in a work of fundamental importance is regrettable. These volumes demonstrate the careful and time-consuming editorial work necessary in such publications, but the delay still seems unduly long. The percentage of mechanical errors is remarkably small.

Frank Freidel and his editorial assistant deserve the hearty thanks of all who labor in the field of American history for providing this basic handbook. It will be as useful to our work as its predecessor—the highest compliment it could be paid. Gratitude must also go to the designer and publisher for making this indispensable reference tool entirely readable.

The *Encyclopedia of American Biography* offers an interesting new wrinkle. Each of the over one thousand entries contains an objective biographical sketch and an interpretive evaluation of the individual's contribution. History graduate students at Columbia University compiled the sketches, and the editors selected appropriate scholars to interpret the significance of the lives, although in many instances they did not choose the most obvious authorities. Decisions about inclusion in the volume rested with the editors, and they were more concerned with an individual's long-range significance than contemporary fame. They freely admit their inclinations to rate artistic and intellectual achievement higher than skills in politics, business, and athletics. Only presidents were assured of inclusion. Many foreign-born persons made the list, but only if their distinctive contributions came during their American years.

Another unusual feature is the incorporation of living figures. If from the perspective of the mid-1970s, a person seemed significant, as did Agnew, Nixon, Kissinger, Landon, and McGovern among politicians, he got included. Among other diverse contemporaries are Simon Kuznets, Elvis Presley, Walter Lippmann, James J. Ling, Michael Harrington, Martha Graham, William Schuman, and Billy Graham. Editorial work on the *Encyclopedia* was amazingly current, witness the inclusion of Arthur Krock's death on April 12, 1974. Insufficient foresight, however, excluded Gerald R. Ford.

The editors have achieved their objectives. Informative sketches give basic biographical facts and the evaluations provide interesting views on their subjects. Sometimes these views appear somewhat skewed, but unorthodox interpretations emphasize the editors' imagination in offering any evaluations at all. The average entry, including both sections, runs around 850 words.

WALTER RUNDLELL, JR.
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College Park

JEROME FINSTER, editor. *The National Archives and Urban Research*. (National Archives Conference, volume 6. Papers and Proceedings of the Conference on the National Archives and Urban Research. Sponsored by the National Archives and Records Service.) Athens: Ohio University Press, 1974. Pp. xii, 164. \$10.00.

This book, presenting the findings of participants in a conference held at the National Archives in June 1970, is a useful but curious potpourri of essays on American urban history and archivists' commentaries on historians' use or neglect of documentary sources in preparing their studies. A forerunner to the panel discussions, a brief plea from Sam Bass Warner, Jr. urges fellow historians to subject themselves to the arduous task of locating and analyzing the facts and figures needed to explain the background of current urban problems so that city planners, administrators, voters, and taxpayers can all gauge the nature and dimensions of the urban crisis. Only attention to "relevance," that overworked term, will permit history, Warner contends, to survive in school and college curriculums.

The four topics chosen for the panels at the 1970 conference were the growth and character of urban population in the United States; past, present, and future housing dilemmas; mass transit problems; and the effect of federal activities on urban development, a theme described by one author as "a Great Impact, a Gingerly Investigation." Although the more than four-year interval between

presentation at the conference and publication of these essays has reduced their importance to scholars—inasmuch as competent new monographs on demographic change, housing conditions, and transportation within metropolitan areas of the United States have multiplied in the interim—the material included in this slim volume will certainly prove invaluable to newcomers to research in what Warner has labeled “The Urban Wilderness.” Nor is it likely that the experienced laborer in the vineyard of urban history will fail to find in the last section of this book helpful guides to information tucked away in the recesses of the National Archives.

This multifaceted collection of treatises is almost as much bibliographical reference as historical exposition. Its various parts are of uneven quality. At best it is far from being easy bedtime reading. But today, when fear of prolonged inflation, severe economic depression, and political discord are casting long shadows over much of the land, the optimism pervading these studies written less than five years ago may encourage enduring faith in our capacity to resolve successfully the problems of “a nation of cities.” If for this reason alone, it is worth looking at.

CONSTANCE MCLAUGHLIN GREEN
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American Narrative Painting. Catalog notes by NANCY WALL MOURE. Essay by DONELSON F. HOOPES. Los Angeles: Los Angeles County Museum of Art, in association with Praeger Publishers, New York. 1974. Pp. 191. \$15.00.

MATTHEW BAIGELL. *The American Scene: American Painting of the 1930's.* (American Art & Artists.) New York: Praeger Publishers. 1974. Pp. 214. \$29.50.

WILLIAM H. GERDTS. *The Great American Nude: A History in Art.* (American Art & Artists.) New York: Praeger Publishers. 1974. Pp. 224. \$25.00.

The idealization of life and experience has played a prominent role in the history of American art, even where the artist aimed at realism; and nowhere is this clearer than in American narrative and figure painting, including the representation of the nude. A reflection, in part, of the belief that art was not meant “merely to give a transient pleasure” but was an instrument of “moral culture” and, in part, of the prevailing nationalism that defined the American character in essentially optimistic terms, such romanticizing resulted—during the nineteenth century at least—in the rejection of the tradition that the expressive force of the figure was beautiful for its own sake and in the consequent emphasis on the story-telling or symbolic element in American painting.

These three books all point up this generalization, as well as provide new ones for the social and cultural historian. Donelson Hoopes's *American Narrative Painting*, a catalog of an exhibition mounted at the Los Angeles County Museum of Art, suggests how the story-telling element in American painting became the “dominant concern of artists of major importance.” Since meaning in human terms becomes inevitably “story,” Hoopes is able to cast a wide net and include in this category works with religious, mythological, historical, anecdotal, and landscape themes. His catalog stops at the twentieth century and the beginning of the prevalence of “abstract” values over literary. The Ashcan School, according to Hoopes, represented “the final burst of energy” of this kind of painting; after the Armory Show of 1913, with such exceptions as the American “regionalists,” the “contemporary narrative painter has labored futilely at an exhausted vein of art.”

Hoopes's short essay provides an adequate summary of his subject. In his eagerness to demonstrate the story-telling propensity of American artists, however, he misses—it seems to me—the essential nature of the narrative painting tradition: its conservatism, idealizations, and agrarian orientation. Not concerned with the relation of these paintings to American social values or with the reasons behind their success in the nineteenth century, or their failure in the twentieth, Hoopes misses the opportunity to make significant distinctions or to probe deeply into the implications of the works he discusses. Since the appeal of many of these works lies more in their social meaning than artistic merit, we are left amused rather than enlightened. Yet, what a mine of material there is here for the social historian!

Matthew Baigell is more concerned with the social—and political—circumstances that underlay the emergence of the American Scene movement of the 1930s—Hoopes's exceptions. He, too, however, is content with description more than with analysis. *The American Scene* concentrates on those artists whose work, Baigell believes, evolved out of the nationalism of the decade—Thomas Hart Benton, Grant Wood, John Stuart Curry, Reginald Marsh, Ben Shahn, and Stuart Davis—and excludes those whose “despair, alienation, and isolation”—Charles Sheeler, Charles Demuth, John Marin—were more “characteristic of the twenties.” Baigell disregards the fact that his aye sayers were as frequently involved in a cynical baiting of the “European-dominated East” with its polyglot populations and “non-American” life styles as they were in affirming their own Americanism against the “foreign” modernists, to whom they owed so much in the way of schooling and actual compositions. When Benton, for example,

inveighed against Eastern cities as the breeding ground for homosexuality and "abstract intellectuality," throwing into one "European" pot all the bitterness and disappointment he had experienced in New York City, and equated regionalism with the effort to "put . . . recognizable American meanings" into painting, he was denying the urban experience created by an industrial civilization and seeking, instead, to return to some pastoral never-never land of the Midwest—that land which the American narrative painters of Hoopes's volume also glorified. Baigell's superficial summary is adequate in its coverage, but misses, as does Hoopes's, the essential nature of the movement he describes. He includes many illustrations in both color and black and white, which in themselves make this a valuable collection.

The same kind of romanticizing that influenced the course of narrative painting in America also influenced the development of the artistic nude, as William Gerdts points out in his thoroughly researched two-hundred-year history of that genre. Also short on social analysis and telling us less about the American character than the dust jacket promises, Gerdts is extensive in his coverage and judicious—at least through the nineteenth century—in his esthetic judgments. He points out that the genre of the nude encountered special problems in the United States that influenced its development specifically. Puritanism—or ideas of sexual morality—of course figures prominently; but also of importance was the absence of schools during the eighteenth and most of the nineteenth centuries, the difficulty of obtaining models, the hovering presence of classical and European styles, and the absence of an artistic philosophy that allowed for the study of the figure for its own sake rather than for purposes of portrait, narrative, or historical representation. Despite these difficulties, as *The Great American Nude* illustrates, some American artists did produce in sculpture and on canvas many beautiful interpretations of the nude. If most of them do not measure up to the standards set by the Great Masters of the past and present, they are surely the equal of the productions of the majority of English and Continental artists who were also not Titians or Manets. If no comparable master of the nude was produced in the United States, surely part of the reason must be in its relatively short history and in the proscriptions imposed by a new society conscious of its moral destiny. Gerdts's book is also richly illustrated and offers a great deal that is pictorially new along with the old favorites.

All three volumes suggest the variety of the American artistic heritage, the extent to which American art historians have succeeded in record-

ing its details, and the great possibilities for analysis that still exist.

LILLIAN B. MILLER
National Portrait Gallery,
Smithsonian Institution

KENNETH A. LOCKRIDGE. *Literacy in Colonial New England: An Enquiry into the Social Context of Literacy in the Early Modern West*. New York: W. W. Norton and Company, 1974. Pp. xii, 164. \$6.95.

The level of literacy in colonial New England was lower than historians of education might lead us to believe, and the growth of literacy was sluggish until the eighteenth century. Even when near-universal male literacy had been achieved the improvement was not associated with attitudinal changes. There was no modernization of consciousness in early America, for the driving force behind the spread of literacy was that traditional agency, the Protestant religion. These are some of the conclusions reached by Professor Lockridge in this very brief introduction to a relatively new area of historical inquiry.

Some readers may be surprised by Lockridge's findings, so the methods by which they were derived deserve careful scrutiny. Evidence for colonial literacy was taken entirely from the ability of testators to sign their wills. The group that Lockridge sampled seems to have been wealthier and of higher status than the colonial population at large, and consequently more literate. On the other hand, their wills were often drawn up in circumstances of illness and old age when the ability to write might have deteriorated. Lockridge claims that "the two sets of biases appear to cancel," but I am not entirely convinced. Nor, one suspects, is Lockridge, for on several occasions he mentions adjustments to his figures, which may be necessary when comparing them to Virginian, English, and Swedish evidence. The description and processing of the data are crucial for the subsequent argument. I am thankful that Lockridge has been forthright about his assumptions and statistical juggling, giving the reader fifteen tables, nineteen graphs, and thirty-five pages of notes to judge the evidence for himself.

The least satisfactory section of this book examines the evidence of charitable giving—again derived from wills—and demonstrates that the era of rising literacy saw no emergence of "modern attitudinal qualities." Bernard Bailyn, Lawrence Cremin, and others who saw in colonial America a dynamic, modernizing society are strongly criticized. It is possible that Lockridge is chasing a chimera, or at least looking in the wrong place. One should not be surprised that when approaching death the thoughts of so many men were "per-

sonal, local, palliative" instead of focused on social improvements. The most significant contribution of this volume has been to indicate the areas that may require further historical revision and to demonstrate that the history of literacy can supply some of the most vital clues.

DAVID CRESSY
Pitzer College

PETER H. WOOD. *Black Majority: Negroes in Colonial South Carolina from 1650 through the Stono Rebellion*. New York: Alfred A. Knopf. 1974. Pp. xxiv, 346, viii. \$10.00.

The importance of slavery in early South Carolina has always been obvious, and there are ample records for its study. Why, then, has the subject been so neglected? Until recently schoolbooks neglected black history, but professional scholars did not. Perhaps they shied away from immersing themselves in plantation life in early South Carolina because it was so generally dreary. Now, by pursuing the contemporary theme of black achievement, Peter H. Wood has found a way to make the depressing material into an exciting book.

Many Africans came from rice-growing regions, but no Englishmen did. It therefore seems likely that blacks, not whites, perfected the cultivation of the colony's first great staple. Blacks were too numerous and socially too isolated from their masters to learn much English from them. They therefore developed Gullah speech from the Atlantic pidgin dialect and borrowings from the other languages, mostly African, spoken in early South Carolina. In the informal, individualistic setting of the frontier, blacks were high achievers in all the agricultural and mechanical arts of the colony, outstripping whites partly because of their superior resistance to malaria and yellow fever. All blacks chafed under oppression, and many struck against it by poisoning, burning, fighting, or escaping their masters. As conditions grew worse, and the Spanish in Florida offered refuge and support, blacks developed large-scale plans for insurrection and escape. These culminated in the Stono rebellion, which, Wood argues, came close to succeeding, but failed. White South Carolinians then strengthened their police, restricted the activities of blacks, cut back new importations from Africa, and inculcated a Christianity that stressed humility and subservience. The early promise and creativity displayed by the black majority—there were roughly forty thousand blacks and twenty thousand whites in South Carolina at the time of the Stono rebellion—became subdued and stifled: "By the time Europeans in America were prepared to throw off the yoke of slavery under which they felt themselves laboring as the subjects of the English king,

the enslaved Negroes in South Carolina were in no position to take advantage of the libertarian rhetoric" (p. 326).

The research behind this book has been thorough and judicious, ranging well beyond the time and place of its main subject to find meaningful clues and wider significance. Wood writes well, and his book has been handsomely produced. Unlike Eugene Genovese, Abraham Lincoln, and several others, he cannot always maintain a proper sympathy for whites as well as blacks. He somewhat exaggerates his case for black achievement, yet it remains formidable. *Black Majority* is a distinguished addition to the growing body of books on colonial slave systems.

ROBERT MCCOLLEY
University of Illinois,
Urbana-Champaign

EDMUND BERKELEY and DOROTHY SMITH BERKELEY. *Dr. John Mitchell: The Man Who Made the Map of North America*. Chapel Hill: University of North Carolina Press. 1974. Pp. xix, 283. \$12.50.

American scholars have toiled long at the task of reconstructing life in colonial America. Of particular interest has been the problem of assessing the role of colonials in the burgeoning global scientific community. The last quarter century has seen a number of works assessing the contributions of previously unknown, or little-known, Americans whose apparent backwoods origins belied their scholarly educations and keen scientific curiosity. Such a book is that on John Mitchell by the husband and wife team, Edmund and Dorothy Berkeley. It is a fine book, well researched and well written, taking its place among the large number of books by the Berkeleys in the history of biology. The authors take full advantage of a variety of sources on both sides of the Atlantic and handle the research material with ease. Arrayed into seventeen chapters with an extensive bibliography, the book deals chronologically with Mitchell's early life in Virginia, his education in Scotland, his medical practice in Virginia, and his later life in England. The emphasis, however, is on his role as an eighteenth-century scientist rather than his own personal life. Consequently the book succeeds less as a biography than as a small window into the scientific world of the eighteenth century. Such famous characters as Benjamin Franklin, John Bartram, Johann Dillenius, Carolus Linnaeus, Mark Catesby, Peter Kalm, and the duke of Argyll stride onto the stage and take their places in the world of Dr. Mitchell. It was a remarkable era peopled by some remarkable men. Intellectuals in the entire Western world were blindly groping for order in science, and much of the stimulus came

from the wealth of previously unknown flora and fauna pouring in from the New World. Well-established rules were constantly being demolished or revised as new specimens were reported, and New World scientists often were instrumental in both discovery and classification. Through the Berkeleys' work one gets exciting glimpses into the problems faced by early taxonomists. Mitchell's role was an important one: he served as a medical doctor, plant collector, political writer, botanist, archivist, and cartographer. He is perhaps best known for his map of North America, which served as the authority in negotiating the treaty of 1783, but his accomplishments were many and varied. He wrote treatises on such diverse subjects as yellow fever, the American opossum, human skin color, and pine trees. He had numerous contacts in both America and Europe, often acting as liaison between eminent European and American scientists of the day.

The book was written by botanists, as it perhaps should have been, and I am pleased with the result, but some will be mildly disappointed. The historian will wish for fuller treatment of the man, while the cartographer will search in vain for details on map compilation. But "satiety for all" is perhaps an impossible goal, thus we must be content with what we have—a very good book.

SAM HILLIARD

Louisiana State University

CATHERINE DRINKER BOWEN. *The Most Dangerous Man in America: Scenes from the Life of Benjamin Franklin*. Boston: Atlantic-Little, Brown. 1974. Pp. xiv, 274. \$8.95.

This is "popular" history-biography. Catherine Drinker Bowen is a great storyteller, and she tells this story with her usual verve and enthusiasm. Not really a biography, the book describes four episodes in Franklin's life prior to the Declaration of Independence: "The Dogood Papers," relating to Franklin's adventures as an anonymous youthful essayist in his brother's paper, the *New England Courant*; "Franklin and Electricity," which sketches Franklin's achievements and fame as a scientist; "The Albany Congress of 1754 and Franklin's Plan of Union," centering about the famous Albany Congress and the plan that Franklin devised for a union of the Anglo-American colonies; "Franklin in London" with three scenes—Franklin's relations with the Pennsylvania proprietors, his involvement in England in the Stamp Act controversy, and his well-known grilling and humiliation by Alexander Wedderburn in the Cockpit on January 29, 1774. There is also an interlude entitled "Franklin is Fifty," describing his activi-

ties in the mid-1750s, and a personal afterword by Mrs. Bowen.

For her material Bowen draws upon other secondary works and Franklin's published letters and papers, especially the autobiography. She uses no footnotes, but she does append a bibliography of printed documents and secondary works.

The book is a very personal piece, broadly dependable as to the major episodes but distressingly casual as to detailed facts and larded with personal generalizations on the English, the Americans, and the human race in general ("What a brute that man [Wedderburn] was!" [p. 248]).

Benjamin Franklin was a man of the common people—the tradesmen, the shopkeepers, the "mechanicks." Yet he was also a man of the Enlightenment, and moved in events and developments of the profoundest importance to Western civilization. Bowen's understanding of Benjamin Franklin is never profound. Neither does she really understand the broad and deep circumstances—social, economic, political, or intellectual—of the Western world in which Franklin lived and worked, or the institutional structure of the British Empire. This book is narration, pure and simple.

Why did she write it? Because she enjoyed doing so. Which is reason enough. Why should anyone read it? Because it is fun. Which, also, is reason enough. But let there be no mistake. This is no contribution either to history or to biography. It adds nothing to what was known before about Franklin, and it fails entirely to qualify as commentary or interpretation. The book will be entertaining, perhaps even valuable, for junior high and high school students; to the patriotic citizen, luxuriating in the ancestor worship of an upcoming bicentennial, it will be highly gratifying; for the serious student of history it is utterly without value.

MAX SAVELLE

University of Washington

PAUL K. CONKIN. *Self-Evident Truths: Being a Discourse on the Origins & Development of the First Principles of American Government—Popular Sovereignty, Natural Rights, and Balance & Separation of Powers*. Bloomington: Indiana University Press. 1974. Pp. xii, 211. \$7.95.

Constitutional questions have been thrust to the center of our attention by the rhetoric of the past two years, and popular reaction to them has revealed us as a politically illiterate people. Nixon used to promote his "new federalism," evoking a response that was emotional and reflexive more often than informed and reflective. The great debate of last summer over separation of powers showed how frequently even thoughtful and well-

meaning people in public life spoke and acted with little comprehension of the key moral and political doctrines underpinning our constitutional government.

Professor Conkin has provided us with a scholarly, perceptive little book about the origins and eighteenth-century meanings and connotations of the basic principles of American constitutionalism: popular sovereignty, natural rights, mixed governmental forms, and balanced separation of powers. Except to a degree in his epilogue, he has not addressed himself directly to the problem of how far removed our political thought and practice are from the forms, intentions, and assumptions of two hundred years ago, but nobody can emerge from a thoughtful reading of *Self-Evident Truths* without realizing that our very frames of reference have changed so much since the eighteenth century that the language of that day carries wholly different connotations today.

Take, for instance, the concept of property. The political system of the founders of the republic, notably John Adams, and of English political philosophers like James Harrington who were fountainheads of American constitutionalism, hung entire political systems on a conception of property as "exclusive claim to a part of nature joined with work, management, and consumption," a conception quite foreign to us today. In this perspective, Conkin writes, "Most of us would be classified as wage earners or mere employees. However well paid, however blessed with the tokens of respectability, we are all still servile men and women, participants in a new and often very paternalistic and benevolent form of feudalism."

Thus for us to appreciate what it meant to assume that property was bracketed with life and liberty as inalienable rights of human beings, we must reckon with the fact that our language and the images it evokes have changed greatly in the last two hundred years.

But the central purpose of this book is to tell where the constitutional principles of 1787 came from and how they evolved. Conkin has achieved this goal persuasively and clearly.

WILLIAM M. DABNEY
University of New Mexico

RICHARD A. BARTLETT. *The New Country: A Social History of the American Frontier, 1776-1890*. New York: Oxford University Press. 1974. Pp. viii, 487. \$15.95.

Richard Bartlett's new book has some of the breadth of his earlier work, *Great Surveys of the American West* (1962), but the present study covers the entire postcolonial frontier and is broadly conceived social history. The author contends that on

the frontier people were freed from social restraints as never before, and the social history of the West becomes the subsequent growth of family, church, school, occupation, transportation, and urbanization out of this near vacuum.

The book begins with a narrative section on the trek across the continent from the breaching of the Appalachians to the census report of 1890. Bartlett believes this sweep to have been inevitable, though it would seem in his discussion that the distribution of the public domain and settlement patterns could frequently have taken different courses. Next comes an analysis of "the basic mix," the people themselves. Here an evaluation of the Scotch-Irish is particularly perceptive. Agriculture is then singled out as the primary frontier occupation. The settlement of the Plains, based on misconceptions stemming from an unusually wet climatic cycle, is compared with earlier eastern frontier farming, as well as with trapping, cattle raising, and mining. Bartlett skillfully surveys the rape of the new country, especially the wanton destruction of the continental forests, both north and south. The chapter on transportation begins with colonial traces, includes such details as the design of Conestoga wagons, and ends with the building of canals and railroads. In an exploration of emerging Western society, the family is the chief concern, with attention to the role of women. Then come also workingmen, transients, churches, schools, and colleges. There are particularly useful sections on the importance of fraternal orders and on the low level of health among most frontiersmen. The final subject is urbanization, in which cities arising from fortuitous sites (Chicago), cattle trails (Dodge City), or mining (Butte) are differentiated from those resulting from boosterism (Los Angeles). Even recreation and entertainment are reviewed briefly as parts of the urbanization of the West.

Bartlett's fine book is not intensely sociological; rather it is social in the tradition of Lewis Atherton and Everett Dick. For example, quantitative analysis of vertical mobility, class structure, or demographic patterns, the kind of studies covered in Ray Billington's *America's Frontier Heritage* (1967), are only incidentally treated. Thus, racial or class frictions become less obvious than Freemasonry and the family. Bartlett is probably correct that such is the way most Westerners would have described their frontier experience.

ROBERT V. HINE
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Riverside*

HAROLD C. SYRETT, editor. *The Papers of Alexander Hamilton*. Volume 20, *January 1796-March 1797*; vol-

ume 21, *April 1797–July 1798*. New York: Columbia University Press. 1974. Pp. xvi, 397; xiv, 360. \$17.50 each.

The volumes of *The Papers of Alexander Hamilton* continue to march with splendid precision from the editorial offices of Professor Harold C. Syrett and his associates. Scholars who have watched the parade will applaud these two additions as enthusiastically as the rest.

As I have noted elsewhere, there has been a marked renaissance of interest in history, and especially in historical sources. A nation become suspicious of its mythology and skeptical of the public pronouncements of its political leaders has shown a decided appetite for such works as Studs Terkel's oral histories. Publishers of the presidential papers of Richard Nixon were astonished at the volume of their sales.

Nonprofessional students will find in *The Papers of Alexander Hamilton* a similar kind of history, but in many ways much more satisfying. Diligent scholarship identifies for them even the most obscure persons referred to, and explanatory notes provide a frame of reference for events discussed in the text of the correspondence. For major episodes there are brief essays that are models for all writers who have struggled to be comprehensive, concise, and literate (see especially the essay on the Farewell Address, vol. 20, pp. 169–73). The editors' achievement is thus both a readable narrative and an invaluable source for professional scholars.

Volume 20 covers a little more than fifteen months from April 1796 to July 1797. During this period the continuing battle over Jay's Treaty moved from the Senate to the House of Representatives. Hamilton assisted the retiring president in hammering out the Farewell Address, and the election of 1796 crystallized the formation of the first party system.

About one-fourth of volume 21 is taken up with the mass of documents connected with Hamilton's entanglement with Maria and James Reynolds. The remainder is concerned primarily with the increasing tension between the United States and France: the XYZ Affair; the tangled complexities of neutral rights on the high seas; and the initial steps taken to meet the problems of internal security and national defense.

Although Hamilton had been out of government for two years, it is obvious that he considered himself still to be "mayor of the palace." When James Monroe was recalled from his post as minister in Paris, Hamilton wrote to the secretary of the treasury, Oliver Wolcott, Jr., "Sometimes I think of sending [Thomas] Pinckney . . . but various uncertainties & possible delays deter me from this plan" (June 15, 1796, vol. 20, p. 224). Nor did the succession of John Adams alter the

situation, as the steady flow of correspondence from Washington's former cabinet officers, retained by the second president, attest (for example, from James McHenry, April 14, 1797, and January 24, 1798, vol. 21, pp. 48–49, 339–40; from Timothy Pickering, March 25, 1798, vol. 21, pp. 371–78).

It is also clear that despite the belligerent, almost warmongering stance assumed by Hamilton in such public utterances as the "Warning" essays (vol. 20, pp. 490–556, *passim*), he could coolly advise McHenry, "The U. States have the strongest motives to avoid war [with France]. They may lose a great deal; they can gain nothing. They may be annoyed much and can annoy comparatively little" (April 29, 1797, vol. 21, p. 63). Thomas Jefferson could not have put it better.

But we still know little of the private man. The dearth of personal letters makes it difficult to penetrate the inner mind and heart of Alexander Hamilton. Did this man of surpassing complexity reveal a somberness of spirit when he wrote in 1797, "Public Office in this Country has few attractions. . . . The opportunity of doing good, from the jealousy of power and spirit of faction, is too small in any station to warrant a long continuation of private sacrifices. . . . The prospect was even bad for gratifying in future the love of Fame, if that passion be the spring of action" (vol. 21, p. 78)?

In these, as in the previous volumes of the *Papers*, the scholarly competence is of an excellence that scarcely warrants the prefatory apology that "the editors on some occasions . . . have been remiss" (vol. 20, p. viii).

JOHN PANCAKE
University of Alabama

DICKSON D. BRUCE, JR. *And They All Sang Hallelujah: Plain-Folk Camp-Meeting Religion, 1800–1845*. Knoxville: University of Tennessee Press. 1974. Pp. xii, 155. \$7.50.

This study stands as one of the promising approaches in the study of American religious history—self-consciously employing interdisciplinary perspectives and focusing on the religious life of lay people, rather than clerical leaders. Winner of the first James Mooney Award (1973), sponsored by the Southern Anthropological Society, Bruce's book analyzes the fascinating and colorful phenomenon of revivalism in the Old South, and his slim, gracefully written volume will provide many challenging arguments for students of Southern history and American religious history.

Bruce maintains that the camp meeting was "first and foremost a religious activity" and that it "can be called without exaggeration a creation of

the plain-folk." Bruce's "plain-folk" are the marginal members of Southern society whose lives were disrupted by high geographical mobility and animated by expectations of social mobility. Despite the hegemony of the planter class, the plain folk of the camp meetings possessed "an urge for respectability" represented by the planters, and though relatively powerless in Southern society, they refused to challenge slavery or the basic deferential values of the Old South. Bruce thus presents an anguished, victimized social group, torn by their aspirations and the reality of their lives, frustrated by an ethic of individualism in an environment that demanded cooperation, and afflicted by random and senseless violence.

Bruce maintains that camp-meeting religion became a creative response by plain folk to deal with the oppressive social reality that they confronted. His most stimulating chapter is an analysis of the camp-meeting hymns, which had their origins largely in Britain and New England but to which the plain folk added their own original and characteristic "spiritual choruses." Bruce interprets the prevailing theme of these choruses to be a total rejection of this world in hopes of a beatified existence beyond the grave. Coupled with the traumatic and publicly narrated experience of conversion, these choruses become symbolic of the attempt by plain folk to create a community of acceptance in a world that had rejected them. Bruce's analysis therefore parallels some of the recent interpretations of slave religion in the South by attempting to go beyond the old "social control" rubric.

Bruce's conclusions, however, cry out for some sort of statistical evidence to substantiate his impressionistic portrait of the plain folk and the constituency of the camp meetings. In addition, his research is disappointingly thin and derivative, but several conceptual problems might have been clarified by attention to additional secondary literature, such as Donald Mathews's interpretation of the second Great Awakening or Sheldon Hackney's work on Southern violence. One is also distressed by Bruce's casual use of the church/sect typology, his continual identification of revivalism with "the frontier," and his shallow theological understanding, but his effort to understand the religious experience of lay people is refreshing, even though his results are not fully satisfying or convincing.

JOHN M. MULDER
Princeton Theological Seminary

WALTER DONALD KRING. *Liberals among the Orthodox: Unitarian Beginnings in New York City, 1819-1839*. Boston: Beacon Press. 1974. Pp. ix, 278. \$12.50.

This is the first of a projected three volumes on the oldest Unitarian congregation in New York City, the Church of All Souls. The author is the minister of the church, and his efforts represent a high order of antiquarianism. He seems to have his own parishioners chiefly in mind as his audience, but his book can be used by historians as well. They will find that it relates the hitherto untold story of the spread of liberal Christianity from New England to New York. It is conveniently documented and nicely illustrated. Biographical data on early members of the society, mostly merchant families who migrated from New England to New York, make the volume a potential resource for social as well as religious history. Among the prominent laity discussed are William Cullen Bryant, Catherine Sedgwick, and Moses Grinnell. Most Unitarians at this time were political Whigs, but a number of those in New York, as in Salem, Massachusetts, and Portsmouth, New Hampshire, turn out to have been Jacksonian Democrats. The most striking character portrayed here is the tragic refugee German liberal, Karl Follen, whose ministry to the parish was cut short in 1838 by his refusal to be silent on slavery. Dr. Kring points with pride to Follen as a hero of his church, but also tries to make excuses for the conservative laymen who drove him out.

DANIEL WALKER HOWE
*University of California,
Los Angeles*

ROBERT C. DICK. *Black Protest: Issues and Tactics*. (Contributions in American Studies, number 14.) Westport, Conn.: Greenwood Press. 1974. Pp. xiii, 338. \$12.95.

Unlike his counterpart in the antebellum South, where the free Negro was regarded as a third element in a system planned for two (in the expressive phrase of U. B. Phillips), the free black in the North could speak out in protest against his lot and in advocacy of reformist measures. Taking advantage of this circumstance, a growing company of black leaders in the North proceeded to express themselves during the forty years preceding the Civil War, their speeches and writings mounting in volume and urgency as their number increased and the times grew more tense. This book successfully undertakes to identify, describe, and analyze the ideas advanced and, to a lesser extent, the tactics considered by these spokesmen. The issues they wrestled with included, among others, voluntary emigration, political party affiliation, racial separation, the use of violence as a means of effecting social change, and, above all, the relative merits of the various ways of striking at slavery.

Meritorious alike in its scholarship and critical analysis, Dick's volume is highly revealing as to the mind of the Negro. In part the ideas expressed by the black theoreticians and expounders he brings under scrutiny were reflective of the reform impulse of the times. Black thought, however, had its points of uniqueness, not only in theme and substance but in its greater sensitivity to the paradox and contradiction in the American creed and in a more unwavering and compelling sense of social urgency. Black ideologues, moreover, like Frederick Douglass and Henry Highland Garnet, were activists as well as theoreticians. For this reason their pronouncements tended closely to reflect the viewpoints of the rank and file. In black life the social and psychological distance between leaders and followers was likely to be minimal. Indeed, as Dick points out, a high percentage of black spokesmen were community-oriented clergymen. Dick also takes note that black opinion makers, although in agreement on ends, often differed as to means. He supports this assertion with a fifty-five page appendix, "Intraracial Issues," of pro and con discourses.

BENJAMIN QUARLES
Morgan State University

EUGENE D. GENOVESE. *Roll, Jordan, Roll: The World the Slaves Made*. New York: Pantheon Books. 1974. Pp. xxii, 823. \$17.50.

Monumental in conception and execution, this book presents a richly documented neo-Marxist analysis of slavery in the Old South. Although the volume is subdivided into four "books" framed by Biblical passages, its structure is triadic, revolving around the dialectically interrelated themes of paternalism, accommodation, and resistance. Book 1 delineates the structural parameters of Southern slavery based on the inextricable paternalistic union of master and slave classes; books 2 and 3 analyze the accommodation of slaves to this reciprocal relationship in their worship, work, status differentiations, domestic institutions, and leisure activities; and book 4 illuminates various forms of resistance to the same paternalistic relationship. Genovese argues persuasively that "accommodation and resistance developed as two forms of a single process by which the slaves accepted what could not be avoided and simultaneously fought . . . for moral as well as physical survival" (p. 658).

Despite its dialectical thematic structure founded on a model of class conflict, *Roll, Jordan, Roll* possesses a strangely static quality that derives primarily from Genovese's mode of using evidence. Within the dynamic framework of his gen-

eral argument, he tends to adduce evidence relevant to specific issues without regard to time or place. Consequently the book is abundantly documented with excerpts and examples drawn from diaries, letters, journals, songs, and oral traditions recorded at various times and places in the Old South, yet it lacks a focused perspective on the development of slavery as an institution. The timelessness of the book is further enhanced by the Biblical passages marking the internal subdivisions of the volume.

Within his panoramic view of "the world the slaves made," Genovese contributes significantly to our understanding of slave life from the perspectives of both slave and master. He not only succeeds in exploding various "legends," but perceptively explores such issues as slave religion, black work ethic, and the role of house servants. Although spatial considerations prevent a detailed discussion of these issues, mention of major highlights of his argument seems useful both for their substantive and methodological import. Conceiving slave religion as a distinctive Afro-American Christian synthesis lacking a "politically militant millennial tradition," Genovese argues that "black Christianity offered profound spiritual strength to a people at bay, but it also imparted a political weakness, which dictated . . . acceptance of the hegemony of the oppressor" (p. 284). In detailing the origin and nature of the black work ethic within the wider context of the Protestant American work ethic, Genovese makes an illuminating contrast between the New World experience of African and European immigrants: "Whereas the Europeans found themselves drawn into an industrial system that slowly transformed them into suitable industrial workers, the Africans found themselves drawn into a plantation system that . . . immensely reinforced traditional values and also added elements of corruption and degradation" (p. 311). In analyzing the role of house servants, Genovese maintains that their intermediary role between other blacks and whites caused them to become "the principal carriers of a third culture, neither African nor Euro-American" (p. 365). Paradoxically, the sociocultural insights suggested by these examples result from the application of the same dialectical interpretive model that contributes to the quality of overall stasis in *Roll, Jordan, Roll*.

While the depiction of the structural and cultural aspects of slave life in the Old South might appear a sufficiently complex analytical problem, Genovese further seeks to place many issues in a larger historical and geographical context. Such comparative discussions are often illuminating but sometimes appear irrelevantly contrived. Thus, while it is surely useful to compare the economic

role of slave children with that of children in European industries of the same period or to explain slave revolts in hemispheric perspective, it seems unnecessary to recount personal experiences of misinformation courteously given by Southern blacks in the 1950s or to mention in passing the importance of granting surnames in feudal Japan. Although such extraneous comparative references are unimportant in themselves, they reflect one cause of the sprawling monumentality of the book that detracts from the presentation of a more dynamic and focused argument.

A more important cause of such monumentality is Genovese's differentiated treatment of structural and cultural issues especially in book 3, which deals with status distinctions, domestic roles, and leisure pursuits. In discussing status differentiations, Genovese begins with structural occupational categories and proceeds to a consideration of cultural distinctions pertaining to free Negroes, miscegenation, and language. Such cultural issues could be more felicitously discussed as aspects of a structural analysis. Such a structural emphasis would contribute to a more effectively focused and succinct analytical argument.

Nevertheless, the virtues of *Roll, Jordan, Roll* deriving essentially from the same neo-Marxist source as its limitations contribute impressively to an enriched appreciation of slave life in the Old South.

MARION KILSON
Radcliffe Institute

ROGER W. LOTCHIN. *San Francisco, 1846-1856: From Hamlet to City*. (The Urban Life in America Series.) New York: Oxford University Press, 1974. Pp. xxii, 406. \$12.50.

In the middle of the nineteenth century, largely as the result of the California gold rush, a modest trading hamlet, Yerba Buena, suddenly became a dynamic, spectacular, cosmopolitan city, renamed San Francisco. Roger W. Lotchin describes the tumultuous and often traumatic events of the first decade of this "instant city." It is a story that should be of great interest to all devotees of the Bay City and could be of considerable value to many students of American urbanization.

As might be expected of a student of Richard Wade, Lotchin draws on a wide variety of contemporary sources to detail the development of the local population, economy, society, government, institutions, and attitudes. Within each of the topical chapters the evidence is presented narratively, with little conceptualization or interpretation. Through this comprehensive portrait of early San Francisco, however, run several important themes: metropolis-hinterland conflicts, the tension be-

tween centralization and decentralization in urban growth, and the perennial search for community. "The attempt to reconcile the rights and interests of the individual and the community," Lotchin concludes, "was probably the single most important thread in the fabric of urban life in San Francisco during the years 1846 through 1856." This is, indeed, one of the central problems in cities, and one appearing most dramatically in boom towns like Lotchin's San Francisco. His analysis, that while people were "very reluctant to act as a community, there was a strong tendency to feel like one," is, however, not very new or useful.

In these "hard times" it is heartening to see published a monograph dealing with a single decade of one city's history; and it is perhaps necessary to have a conclusion drawing the contemporary relevance of the subject. It is unfortunate, however, that Lotchin emphasized the antithetical relationship between San Francisco and contemporary "instant cities," like Irving Ranch, at the expense of a more detailed examination of the similarities with Carl Bridenbaugh's colonial cities and Wade's river cities.

WALTER S. GLAZER
University of Pittsburgh

JONATHAN KATZ. *Resistance at Christiana: The Fugitive Slave Rebellion, Christiana, Pennsylvania, September 11, 1851. A Documentary Account*. New York: Thomas Y. Crowell, 1974. Pp. viii, 359. \$7.95.

On September 11, 1851, a Maryland slaveowner, accompanied by a federal marshal and a posse, tried to capture several of his fugitive slaves at a rented farmhouse near Christiana, Pennsylvania. Standing against this posse were a group of free and fugitive blacks. In the melee that followed, the slaveowner was killed and several blacks and whites were wounded.

Jonathan Katz has ably captured the drama of this resistance and its aftermath. The hero of the piece is William Parker, a Maryland fugitive who had organized the Christiana blacks into a self-defense organization for protection against kidnappers as well as slave hunters. Katz convincingly minimizes white participation in this organization and in the later resistance. At the inquest and trial following the battle, three Quakers were charged with inciting the fugitives to riot and indicted for treason. Katz argues that the Quakers' commitment to pacifism prevented them from sanctioning violent resistance. They would help the blacks to run, but not to stay and fight. The Quakers' trial took nearly a month. The jury was more decisive. In fifteen minutes, it returned a verdict of not guilty. Two weeks later the local

district attorney dismissed charges against the blacks arrested after the battle on the grounds of insufficient evidence. Meanwhile, William Parker and several other resisters had fled to Canada. There they remained.

The book is mistakenly subtitled "a documentary account." Katz actually strings together lengthy quotations with his own narrative. While this technique may give the reader a feeling of objectivity as well as evoke "an immediate sense of this historic event," it often confuses and disturbs the flow of the story. Katz tells us more than we need to know. There are too many quotations and too much undigested information. When he is on his own, Katz's prose is vivid and the tale absorbing.

This is an important book. Painstakingly researched, it is obviously a labor of love. One can feel in reading it the terrible desperation of the fugitives and their incredible determination to be free. Katz sees them as authentic heroes, and he is absolutely right.

PETER M. MITCHELL
Seton Hall University

DONALD R. WARREN. *To Enforce Education: A History of the Founding Years of the United States Office of Education*. Detroit: Wayne State University Press. 1974. Pp. 239. \$11.95.

"Resolved, that the Joint Committee on Reconstruction be instructed to inquire into the expediency of establishing in this capital a national bureau of education, whose duty it shall be to enforce education without regard to race or color, upon all such States as shall fall below a standard." This House resolution, moved in December 1865 by Ignatius Donnelly, well exemplifies the minor, but revealing national movement that is the subject of Donald Warren's book on the founding and early years of what is now the United States Office of Education (USOE). The movement reflected the classic paradox, and the American response to it, that underlies compulsory education: we must compel citizens to learn in order to make them free. For men from Benjamin Rush to Donnelly, the paradox had political meaning: free republican society would endure only if its young were taught the values of that society. It is no surprise that Donnelly's resolution and the creation of a national bureau followed the Civil War. "Enforce," however, was strong medicine, and while it had the blessing of influential educators, the resolution went beyond what Congress could in fact tolerate. In the educational domain states' rights were yet paramount. Warren's book traces the idea of a national responsibility for education and the efforts to translate this into a federal in-

stitution. National education found its earliest form in the informal association of practicing schoolmen. Though by 1867 these men may have wished for some nationally enforceable school standards, the bureau, which Congress finally created for them, was little more than an extension of their association—an office to collect statistics and publish reports.

Warren has taken what at first blush could be considered a yawningly bland topic and makes it a useful addition to the growing number of studies that show how nineteenth-century Americans endeavored to create a "top-down" educational system—one which reflected and reinforced majoritarian values. Fortunately the self-righteousness all too common in current revisionist educational history is pleasantly absent from this study. Unfortunately though, the sections on the early years of the bureau are somewhat repetitious, and the final chapter is a strained attempt at relating the early history of the bureau to the current struggles of USOE and the National Institute of Education. In all, however, *To Enforce Education* usefully shows the early handling of the classic educational paradox. It is a story that has poignant meaning for school systems and colleges, which today are faced with various kinds of federal enforcements.

THEODORE R. SIZER
Phillips Academy

R. GORDON KELLY. *Mother Was a Lady: Self and Society in Selected American Children's Periodicals, 1865-1890*. (Contributions in American Studies, number 12.) Westport, Conn.: Greenwood Press. 1974. Pp. xx. 233. \$12.50.

This is a study of major themes of stories in the famous American children's magazines of the Gilded Age. After a prosaic discussion of the "institutional matrix" of post-Civil War magazine publishing and an awkward presentation of "formulas" for cultural and content analysis come five better chapters on key moral problems of American life that were reflected in fiction for youth. Kelly's main hypothesis is that the precarious position of an American gentry under industrialization and urbanization led to a search for a moral order that would act to deny dangers or would reassert the power of older ideals. The analysis of the stories themselves from *Saint Nicholas*, *Our Young Folks*, *The Youth's Companion*, and others, is most engaging and suggests how much a sensitive imagination can still contribute to the writing of social and cultural history. Kelly, however, seems under such strong obligation to discuss methodology that, inescapably, he must be judged on his constructs as well as his sensitivity. Con-

cepts drawn, hither and yon, from sociology, content analysis, and other disciplines are used with ungraceful self-consciousness and often seem intrusive and too much at odds with his considerable talent for condensing and analyzing the stories.

Use of the term "gentry" further confuses scholarly debate about its rise and fall. Kelly adds to this problem by recurrently using words like "gentility," "gentleman," and "genteel" as interchangeable with "gentry." His own evidence suggests that moral and psychological concerns transcending class underlay these stories, and his attempt to link the general anxiety of an age to a social and political entity like gentry weakens his analysis.

Kelly seems to pull in such large-scale phenomena as industrialization and urbanization to explain the sense of fear and danger about gentry ideals. Insofar as a sense of crisis for character or for the self has been almost continuous throughout American history, urbanization and industrialization may not explain as much as Kelly seems to think they do. His references to Darwinism and changes in moral beliefs and religious and family life seem more fruitful for understanding the dilemmas and ideals that were depicted for children. Kelly's study has most value as a reading of these tales rather than as an explanation of the meaning of their appearance.

BERNARD W. WISHY
North Carolina State University

HANS-ULRICH WEHLER. *Der Aufstieg des amerikanischen Imperialismus: Studien zur Entwicklung des Imperium Americanum, 1865-1900.* (Kritische Studien zur Geschichtswissenschaft, number 10.) Göttingen: Vandenhoeck & Ruprecht. 1974. Pp. 426 DM 49.

In this stimulating analysis of the rise of American imperialism, Hans-Ulrich Wehler, a leading German historian, applies to the United States the interpretive framework that he has used in his study of Germany. He views modern American imperialism, like Bismarck's, as the attempt by the ruling elite to escape from the social and economic, and consequently political, problems inherent in industrial capitalism. Although noting other factors such as racial and religious beliefs in the ideology of American expansion, he emphasizes the economic motivation of what he calls social imperialism. He interprets the American drive for markets in Latin America and Asia as the response by Americans to the depressions and social unrest of the late nineteenth century. This interpretation is not altogether convincing. The American desire for foreign markets, especially for agricultural products, existed before the development of industry; it was more traditional, and less the prod-

uct of a highly industrialized society, than Wehler admits. The American economy, moreover, never depended upon foreign trade to the extent that he claims; overseas markets, although highly prized, were not always the controlling factor in American foreign relations even after the closing of the frontier and the emergence of industry.

Although frequently noting that the United States reacted to British, German, and Japanese activity in Latin America and the Pacific, he does not adequately account for this information in his interpretation of American motivation. Wehler's belief in the primacy of internal politics guided his research as well; he examined numerous American manuscripts and archives but neglected foreign sources for this book. Rather than exploring the complex interaction between international and domestic influences on foreign policy, he views imperialism—whether German or American—from a nationalist perspective. Despite these limitations, Wehler's book is a subtle piece of scholarship that deserves the attention of American historians. It is a major contribution to the revisionist literature on American foreign relations.

LLOYD E. AMBROSIUS
University of Nebraska,
Lincoln

ALAN HYNDING. *The Public Life of Eugene Semple: Promoter and Politician of the Pacific Northwest.* Seattle: University of Washington Press. 1973. Pp. xiv, 195. \$10.00.

Historian Daniel J. Boorstin has described him as the man with the "go-getter" spirit, symbol of late nineteenth-century America. Professor Alan Hynding's well-researched, well-written biography of Eugene Semple once again confirms how central to that period was the role of the impetuous businessman-promoter. Farmer, journalist, lawyer, politician, and speculator, the entrepreneur Semple followed a well-worn path.

Most such Beriah Seller figures never arrived at even the ephemeral prominence gained by Semple. In this instance, as the biographer Hynding readily concedes, "The Eugene Semple Papers constitute the most important single source for the study, and indeed, its justification" (p. 173). This extensive collection at the University of Washington documents almost every phase of Semple's varied career from the time of his arrival in Oregon in 1863, at the age of twenty-three, until his death in 1908. Like Mark Twain, Semple was among those Americans who preferred the smell of Far West adventure to Civil War battle smoke.

The Pacific Northwest's lush farmlands, forests, and especially its burgeoning port villages invited the frontier dreamer to "grow with the country."

By 1866 Semple had inaugurated Portland's first Democratic party daily. Across his columns he chased Negroes, Chinese, and monopolists. His entry into Washington Territory business and politics was well launched by the time his Oregon *Daily Herald* collapsed in 1873. Lumber mill operator, amateur engineer, governor of Washington Territory during the tempestuous late eighties, and forever with an eye out for a rewarding commercial venture, Semple personified the educated civic booster whose accomplishments fused public welfare with private profit.

Hynding has tightly woven his colorful subject into the regional garment. Certainly both Pacific Northwest labor and conservation historians will wish to examine this study. In particular, urbanologists will benefit from the biographer's detailed analysis of how Seattle's city fathers shouldered away hills, tidelands, and each other in building Puget Sound's pre-eminent city.

On his death Semple left his family no tangible legacy. Indeed, in his late years he had to confess, "I am willing to endure almost anything in the shape of danger or deprivation in order to make some money" (p. 172). Never as powerful as Washington's Thomas Burke or as flamboyant as Seattle's Erastus Brainerd, Semple's campaign to connect Lake Washington to the sound with a ship canal, his sensible if visionary views to develop the Seattle waterfront, as well as a number of other public-spirited activities entitled him to a more serene old age. Happily Professor Hynding's skill has supplied the promoter-politician an immortality that his contemporaries could not gauge.

TED C. HINCKLEY
San José State University

JAMES EDWARD WRIGHT. *The Politics of Populism: Dissent in Colorado*. (Yale Western Americana Series, 25.) New Haven: Yale University Press, 1974. Pp. xii, 314. \$16.50.

Despite Colorado's importance as a mining, agricultural and urban center in the late nineteenth-century West, the state's politics have not received systematic treatment. With figures like Henry M. Teller, Edward O. Wolcott, and the Populist governor Davis H. Waite on the scene, this neglect was puzzling. James Edward Wright has now examined the political culture during the four decades after Colorado joined the Union in 1876 to demonstrate that "Populism had roots deep in the social and economic development of the state." Wright's study presents much valuable material on the behavior of voters in the Centennial State, but the narrow focus of his research in manuscript sources limits the book's usefulness as a guide to the history of Colorado's political parties.

Wright is most comfortable and assured with election data, and the careful work in these chapters indicates strongly that Colorado residents found economic issues more salient influences than the ethnocultural forces so powerful in the Midwest. Unlike Karel Bicha, whose article of early 1973 he does not cite, the author did not find Colorado Populists conservative but rather advocates of "a strong exercise of power by the federal government." Critical of the failings of the major parties, Wright is generally sympathetic to Populist programs and leaders like Waite. "Colorado Populism," he concludes, "clearly was not a retrogressive force seeking to halt progress and industrial development."

Because Colorado politicians between 1877 and 1901 left relatively sparse manuscript collections, it is necessary to go beyond the state's borders and the material local archivists have preserved. Wright did not do this and, accordingly, his account of political maneuvering is thin. The Grover Cleveland Papers and the Justice Department Appointment Papers in the National Archives, for example, would have confirmed his surmise about a Populist-Democratic arrangement on fusion in 1892 and the subsequent repercussions on the Democrats between 1893 and 1895. There are Colorado letters in the William Jennings Bryan Papers about 1896, and evidence on Populist relationships in the Ignatius Donnelly and Marion Butler Papers. Edward O. Wolcott corresponded with Eastern Republicans like Henry Cabot Lodge and William E. Chandler; the Benjamin Harrison Papers have a good deal on GOP factionalism in the early 1890s. The absence of Thomas Dawson's biography of Wolcott from the bibliography is odd. Without such manuscript research, Wright's account of party activity in key areas often has to rely on newspapers and, in many instances, older dissertations about the state.

Wright's book is a competent start on Colorado politics in the Gilded Age, particularly for the electoral evidence. It would have been better had he taken the time to consult the available primary sources in national repositories and to integrate their information into his story. As it stands, this volume is more than the dissertation from which it derived, but less than a well-researched, carefully drawn analysis of a significant Western state in the midst of Populist turbulence.

LEWIS L. GOULD
University of Texas,
Austin

GERALD P. FOGARTY, S. J. *The Vatican and the Americanist Crisis: Denis J. O'Connell, American Agent in Rome, 1885-1903*. (Miscellanea Historiae Pontificiae, volume 36.) Rome: Università Gregoriana Editrice, 1974. Pp. x, 357. L. 7,000.

American Catholicism has never been the influence in national life that might be expected from its size, centralized organization, and wealth. Forced on the defensive by nineteenth-century nativism, it entered a long external struggle to overcome animosity and prejudice. The Church weakened itself, however, by an equally long struggle concerning a policy of adapting itself to the American traditions of pluralism and separation of Church and state. The result of both struggles was a Catholic ghetto mentality, which still lingers on well past the mid-twentieth century. Occasionally, bold efforts were made to break this pattern and to effect a broader outlook on American society. These efforts engendered a bitter debate among the Church's hierarchy, which weakened effective leadership and scarred the careers of its participants. With issues never really resolved, the entire debate was silenced by Leo XIII's encyclical *Testem Benevolentiae*, ending what has since been called the "Americanist crisis." Historians have not fully analyzed the deep-seated impact of the total debate on the Catholic contribution to American life. Thomas McAvoy's *The Great Crisis in American Catholicism* (1958) carefully presents the debate at its height in the 1890s. A few biographies, most notably John Tracy Ellis's *The Life of James Cardinal Gibbons* (1952), offer glimpses at the depth of the resulting bitterness. Fogarty here clearly underscores this aspect.

Denis O'Connell, considered a liberal among late nineteenth-century clerics, was never really a leader of that position. A protégé of Cardinal Gibbons and a confidant of Archbishop John Ireland, O'Connell was appointed rector of the American College in Rome and thus became the unofficial spokesman for the American clergy at the Vatican. His role in the Americanist debate was significant, however, for through him the liberals' views were channeled to proper authorities, while those of the opposition were filtered out.

Fogarty's treatment of O'Connell is, however, disappointing. Admitting his unwillingness to present a total biography, the author focuses his attention on O'Connell's years in Rome. This could have been most satisfactory if he had adequately understood the major issues causing the crisis. But, with confusing chronology, he quickly gets lost in clashes of personality, minimizing the broader context of meaningful and valid differing points of view. At best, he finds the chief source of difficulty to be an irreconcilable German-Irish conflict. Furthermore he almost entirely ignores nonreligious documentary collections and lay authors and thereby writes another piece of institutional history from a narrow perspective. This also is unfortunate, for too much Catholic

historiography has already been offered from this perspective. Despite limited success in achieving its potential, the Catholic Church in the United States has made a rich, meaningful contribution to American society. Fogarty's book adds little to the understanding of that contribution.

FRANK T. REUTER

Texas Christian University

LOUIS R. HARLAN *et al.*, editors. *The Booker T. Washington Papers*. Volume 3, 1889-95. Urbana: University of Illinois Press. 1974. Pp. xxx, 618. \$17.50.

In 1889 Booker T. Washington's personal life was in its severest crisis because of the death of his second wife and closest co-worker, Olivia A. Davidson. In 1895 his public life reached its climax when he delivered the "Atlanta Compromise" speech. By choosing 1889 and 1895 as the terminal points of volume 3 of *The Booker T. Washington Papers*, Louis R. Harlan emphasizes both Washington's achievement and the roles he had to play to become America's most famous black leader from 1895 to 1915. These papers show that both the images and ideas in the Atlanta speech were present in Washington's mind long before 1895. If Washington first heard the "separate as the fingers" metaphor in 1880 (p. 582), he certainly practiced and preached the doctrines of accommodation, self-help, and "industrial" (vocational) education as the proper strategy for racial advancement from Tuskegee's start in 1881. While Washington expanded the circle of his acquaintances among America's leaders from 1889 to 1895, he centered his efforts on developing Tuskegee. His letters show his ability and determination always to oversee the minute details in Tuskegee's operation.

Harlan's comprehensive, exhaustively researched biographical annotations and detailed index seem destined to make this series of edited papers both a model for historians and a basic source for any study of American culture from 1856 to 1915. Ironically Harlan's editorial excellence causes some minor concern. In making this volume so readable he omitted several items that social historians would find useful. Harlan's focus on Washington justifies almost all omissions except perhaps I. Garland Penn's letters of July 8 and 17, 1895, seeking a job at Tuskegee, which might help explain Penn's diligence in securing Washington's invitation to speak in Atlanta. The quality and quantity of this edition of Washington's papers, and Harlan's biography of Washington, might cause some readers to over-stress the influence of Washington and his philosophy. To provide a balanced view of the "age of Washington" historians should treat Harlan's out-

standing achievement as a challenge to prepare works of similar quality on other black and white leaders who espoused opposing opinions.

ROBERT G. SHERER
Wiley College

THOMAS S. HINES. *Burnham of Chicago: Architect and Planner*. New York: Oxford University Press. 1974. Pp. xxiii, 445. \$19.50.

"Make no little plans," said Daniel H. Burnham; "They have no power to stir men's blood." Whether designing the innovative Montauk, Monadnock, or Reliance buildings, serving as director of works for the World's Columbian Exposition in 1893, or preparing comprehensive plans for the rebuilding of large parts of Washington, Cleveland, San Francisco, and Chicago, Burnham followed this credo throughout a diversified and extraordinarily productive life. At the time of his death in 1912, he was the most famous architect in America and a city planner of international renown; as a builder and shaper of urban communities, his only rivals in the past century are Frederick Law Olmsted and Robert Moses.

Since 1912, however, Burnham's reputation has been somewhat in eclipse, in part because the human costs of redevelopment have soured our taste for grand designs and in part because architectural critics have recognized the superior genius of Frank Lloyd Wright and Louis Sullivan. To his credit, Thomas S. Hines does not claim that Burnham's artistry exceeded that of his Chicago contemporaries—Sullivan himself disliked Burnham and Wright refused his offer of a job. But Hines makes a convincing case for the proposition that Burnham was a "great" man whose achievements as an architect, planner, administrator, and philanthropist were both original and distinguished. Burnham also seems to have been a good and decent man who was helpful and loyal to dozens of causes and individuals.

Aside from the illustrations, which though numerous did not reproduce well, Hines has provided an intelligently organized, well-written, and fascinating account of Burnham's entire career. The analysis of Burnham's much discussed relationship with his brilliant partner, John Wellborn Root, is perceptive and well balanced, and the criticism and comment on the various buildings and plans are sensible and readable. Many of the individual chapters in the volume have been or could be the subjects of entire books—the Columbian Exposition, the rise of the skyscraper, the plans for Washington and Chicago, for example—and thus it cannot be said that Hines has offered the last word on these topics. But *Burnham of Chi-*

cago is an excellent book and a model of scholarship, good judgment, and literary grace that satisfies a long-felt need.

KENNETH T. JACKSON
Columbia University

J. NOEL HEERMANCE. *Charles W. Chesnutt, America's First Great Black Novelist*. [Hamden, Conn.:] Archon Books. 1974. Pp. xiii, 258. \$12.50.

Chesnutt was a Negro who appeared white. He spent fourteen youthful years in North Carolina, then worked a half-century in Cleveland. He wrote some eighty stories and at least nine novels, three of which were published. Concurrently he produced essays, speeches, and correspondences with prominent persons on racial topics, a legacy of nonfiction that is still largely unpublished. This output was devoted to advancing the interests of black people; it should be known in a nation beset with racism. Heermance provides an introduction to a neglected figure of historical importance.

The title suggests that Heermance intended to focus on Chesnutt's novels, but the opening review of some of the scholarship on racial questions is insufficient as background for literary comment. Nor does the writer indicate familiarity with Sylvia Render's 1962 dissertation on Chesnutt's fiction. If the intent was psychohistory or biography the author needs further research, partly to fill gaps between 1905 when the third novel appeared and 1932 when Chesnutt died. In a sense Heermance terminated Chesnutt's career with the third published novel. Chesnutt was a force in Cleveland for much longer; through memorable stands he subsequently attained national significance.

So the book is an overview of some high points in Chesnutt's career. It deals with Chesnutt's fiction, mentioning peripherally, often unchronologically, the nonfiction. Heermance suggests Chesnutt's commitment was basically nonracial. He relies heavily on the sentimental biography by Chesnutt's daughter and derives most of his footnotes from the Fisk Collection. But even in these materials there is much evidence supporting Chesnutt's early racial commitment.

Had Heermance explored papers at the Western Reserve Historical Society and other sources in the community where Chesnutt lived he might have revised his interpretations that Chesnutt ever "chose and staked out a position of nonracial isolation." W. E. B. DuBois disagreed too. In 1928 he nominated Chesnutt for the NAACP's Spingarn Award.

FRANCES RICHARDSON KELLER
California State University,
San José

THOMAS B. TURNER. *Heritage of Excellence: The Johns Hopkins Medical Institutions, 1914-1947*. Baltimore: Johns Hopkins University Press. 1974. Pp. viii, 648. \$17.50.

In 1943 Dr. Alan Chesney published the first of three volumes dealing with the history of the Johns Hopkins Hospital and Medical School from its origins to 1914. Dr. Thomas Turner's *Heritage of Excellence* carries this history forward to 1947. Turner, like Chesney before him, is a distinguished microbiologist and dean emeritus of the Johns Hopkins Medical School.

Although Turner's history focuses on such institutional developments as the organization and growth of the School of Hygiene and Public Health, the Wilmer Ophthalmological Institute, and the Institute for the History of Medicine, he also examines in detail the inauguration and maturation of the full-time teaching system and its effect on specialization, as well as various aspects of the life of medical students, interns, and young physicians. Turner is especially acute in delineating the administrative and financial history of the various Hopkins medical institutions, and reveals the internal history of major medical school and hospital departments in a series of informed biographical sketches of many Hopkins notables. While Turner celebrates the achievements of the Johns Hopkins Medical School and Hospital, he does not shun critical examination of administrative mistakes, from important developmental plans that died a-borning, to the shoddy, pre-emptory dismissal in 1926 of Dr. Ralph Bowers, a talented surgical intern caught smoking in the lunchroom.

Despite its general excellence, Turner's history is not without fault. It would have been helpful, for example, if he had given more precise references to the archival documents he used and quoted. Problems of a more substantive nature are to be found in some of Turner's interpretations, particularly in his treatment of the later career of Dr. Henry Sigerist, the *doyen* of medical history in the United States. There can be no doubt that Sigerist was a trial to the administrators of the Johns Hopkins Medical School for his outspoken observations on the social problems of medicine. Turner, who appreciates Sigerist's genius, nevertheless sees Sigerist's concern with such problems as alien to his function as a medical historian, and more darkly, as the result of his trips to the Soviet Union.

Actually, Sigerist's concern with the social problems of medicine came less from his trips to the Soviet Union than from his vision of the function of medical history. He expressed those views most clearly in his talk "Medical History in the United States," which he gave to the Johns Hopkins Medical History Club in May of 1947. "The history of medicine is both history and medicine. It is one

aspect of the history of civilization and part of the theory of medicine. The historical analysis is a method that can be applied profitably in medicine as in other fields, to clarify concepts to make trends and development conscious so that we may face them openly and may act more intelligently. When you pursue your historical studies into the present, you imperceptibly enter the field of sociology and begin to see that an infinite number of non-scientific factors, social, economic, political, philosophical, religious, may well determine success or failure of medicine, factors which must be investigated."

Such criticism aside, Dr. Turner's history is an earnest of the important contribution that physicians and scientists can make to the writing of the history of medicine.

SAUL BENISON
University of Cincinnati

MIRA WILKINS. *The Maturing of Multinational Enterprise: American Business Abroad from 1914 to 1970*. (Harvard Studies in Business History, 27.) Cambridge, Mass.: Harvard University Press. 1974. Pp. xvi, 590. \$22.00.

In this second volume of her history of American multinational corporations, Mira Wilkins has undertaken an awesome task. American business abroad has grown tremendously in the present century, and the nation's role in international affairs has changed in numerous and complex ways—most of which have influenced the affairs of our multinational businesses. Moreover, the quarter of a century since the end of the Second World War is still an open frontier of research. Diplomatic and business historians are just beginning to churn out the carefully documented monographic studies that exist in abundance for earlier periods in our history.

Despite the difficulties of her job, the author has written a creditable survey. This is the best single volume I have read on this important subject. The book is based on careful research in primary as well as secondary materials; the author also conducted numerous interviews in an effort to fill the gaps in the documentary record. Even those scholars who feel oppressed when they find footnotes at the end of a book will discover that Wilkins's elaborate notes are worthy of their attention.

Intellectually this study is more venturesome than was the author's previous volume, *The Emergence of Multinational Enterprise* (1970). Wilkins acknowledges that the activities of American corporations overseas have aroused various, conflicting responses. Not all have been enamored of the behavior of Standard Oil, and even the performance of ITT has sometimes invited suspicion. New Left

analysts, in particular, have proclaimed that our foreign policy has all too often yielded the public interest to the private needs of multinational enterprises. Wilkins carefully considers these arguments and debates the issues in her text and notes. Her conclusions are, for the most part, consistent with those advanced in her previous book and are not likely to disturb the sleep of many corporate executives. Nevertheless, we can be grateful for her willingness to meet head on these difficult questions.

On balance, however, most readers will probably find the information in this volume more useful than its interpretations. As Alfred D. Chandler, Jr. points out in an editorial introduction, the author's major contribution is "getting the chronological record straight" (p. vi). She has not developed any startling new insights, nor has she offered us any general synthesis. She has surveyed important ground and cleared away a bit of the brush, leaving to others the job of tilling the soil and harvesting the first crop of new historical knowledge.

LOUIS GALAMBOS
Johns Hopkins University

ZOSA SZAJKOWSKI. *Jews, Wars, and Communism. Volume 2. The Impact of the 1919-20 Red Scare on American Jewish Life.* New York: Ktav Publishing House, 1974. Pp. 398. \$20.00.

This book attempts to consider the impact on American-Jewish life of the wave of xenophobic hysteria at the end of World War I. Szajkowski gives particular attention to how Jewish radicals fell under attack during the "red scare," and how Jewish spokesmen of all shades of opinion responded in debates over labor relations, Americanization, the proper defense against growing anti-Semitism, and the communal self-examination that the hostile climate forced upon American Jewry.

Not surprisingly, this second volume of Szajkowski's work shares many characteristics with the first. He is once again eager to do battle with "the Jew-Bolshevik myth"—the dangerous notion that untold numbers of Jews were committed radicals, busily plotting against the nation. Once again he insists that "there was no monolithic Jewish attitude either to the war in general or to various anti-liberal measures adopted by the Washington or local administrations" (p. 4). And once again readers are left wondering what serious student of American-Jewish attitudes has ever posited a monolithic Jewish view.

The book also, unfortunately, shares with its predecessor a set of annoying weaknesses. There are numerous small and inconsequential errors. The book is poorly put together and repetitious—

in one memorable instance, the same quotation appears twice within the same paragraph (pp. 8-9). It is a scissors-and-paste collection of undigested research notes, loosely tied together by introductory sentences; between pages 58 and 68, the author contributes fewer than forty original lines, leaving his dozens of quotations to account for all the rest. It is a rare and restful chapter that actually discusses, systematically and without interruption, exactly what the chapter title promised. But above all, it is all combined without judgment or evaluation—the views of Felix Frankfurter, for example, will be blithely counterbalanced by the views of someone named Isse Koch, as if discovering Jewish attitudes was simply a matter of lining up all the Jews who ever expressed an opinion and quoting one after the next. It is the lack of judgment, the unwillingness to decide what is important and what is trivial, that kills the book.

But there are treasures in it too—marvelous quotations, moving stories, fascinating episodes, colorful personalities. They are gathered from Szajkowski's relentless digging into a wide range of documentary materials, and readers will doubtless be less disappointed if they approach the book as an absorbing collection of research materials rather than as an enlightening or useful scholarly interpretation.

DAVID W. LEVY
University of Oklahoma

THOMAS B. BUELL. *The Quiet Warrior: A Biography of Admiral Raymond A. Spruance.* Boston: Little, Brown and Company, 1974. Pp. xviii, 486. \$15.00.

The Quiet Warrior is an apt title for this highly readable biography of Admiral Raymond A. Spruance, leader of the U.S. Navy's decisive drive across the Central Pacific in the war against Japan. A modest man, Spruance defied the efforts of contemporary journalists to glamorize his performance as hero of Midway and commander of the U.S. Fifth Fleet from the capture of Tarawa to the invasion of Okinawa. It is to Commander Buell's credit that, despite the sparseness of the written record, he has pieced together enough details about his subject's personal life and naval career to produce a scholarly full-length biography.

To do so Buell has relied heavily, perhaps too heavily, on interviews with surviving family and friends and on oral histories, especially the lengthy memoir of Spruance's wartime chief of staff, Carl Moore, in the Columbia University Oral History Collection. Whatever his sources, Buell's judgments on Spruance as fleet commander are his own and are not uncritical. He faults the admiral for his failure to launch search planes on the first afternoon of the battle of Midway, and, more tell-

ingly, for his refusal to allow Marc Mitscher's fast carriers to pursue the Japanese carrier fleet in the early stages of the battle of the Philippine Sea for fear of an end run that might endanger the landing operations on Saipan. In general the author tends to agree with Admiral Mitscher and other naval aviators that Spruance, a battleship sailor of the old school, was too cautious and did not fully appreciate the potentialities of carrier aviation.

Nevertheless, Spruance's reputation as one of the naval greats of World War II is reinforced by this volume. His was a greatness composed of professional expertise, sound intuitive judgment, skill at picking subordinates and willingness to delegate responsibility to them, personal integrity, bravery, and above all imperturbability. The quiet warrior ran a quiet bridge. Only a professional sailor like Buell could fully appreciate the significance of this fact. In this as in other matters, the author's own experience as a naval officer serves to lend authenticity to this admiring, though not adulatory, biography.

PHILIP A. CROWL
Naval War College

MARTIN BLUMENSON. *The Patton Papers: 1940-1945*. Boston: Houghton Mifflin Company. 1974. Pp. xix, 889. \$20.00.

This has to be as fascinating a book on a military figure as has been printed. The core of it is a diary kept by George S. Patton with letters to and from him wound around that core and with perceptive comments inserted by Martin Blumenston. The end product is so candid that the reader can only be proud of Patton's descendants for agreeing to make it public. Patton, like most diarists, used his diary to ease his own frustrations. This he achieved, as the rest of us do, by scaling down the people who produced his tensions. Of Montgomery he wrote, "I can outfight the little fart any time" (p. 359). He was often hard on Eisenhower, too, since Eisenhower, who had been his junior for many years, soared past him to the top. At all times Patton considered Eisenhower a parlor-style officer who had never known combat. "I wish to God he was more of a soldier and less of a politician" (p. 417). Walter Bedell Smith never rose above the s.o.b. level in the diary. Recording the few meetings he had with FDR, Patton spoke only good of the president and had nothing to say against General Marshall except that he, like Eisenhower, had never known war down where the blood flowed. Patton resented the managers who had replaced the heroes in the forefront of war.

Nevertheless, being through and through a soldier, he always carried out orders, whatever he thought of their issuers. Moreover, being ambitious, he took pains to keep on the good side of

his commanders. The contrast between what he recorded in his diary about people and what he said to them personally reveals hypocrisy, but no more than the average content of it. The hypocrisy stands out because Patton's private opinions are here placed in print beside his public utterances. Hardly any public figure could pass such a test untainted. In Patton's case, history is the richer.

So human is this personal story that the reader can hate almost everything Patton stood for, yet, knowing how he felt, yearn for him to achieve fame and glory, which he sought above all. I am personally revolted by his continual exhortation to his troop to kill. The noblest work of God, he told them, was the killer, and his diary entry on Christmas 1944 said, "A clear cold Christmas, lovely weather for killing Germans" (p. 606). In the next clause he recognized the incongruity of this, but was in no way upset. He often read the Bible, and his God was Jehovah of the Old Testament. To him he prayed, "I am the best there is, but Lord give us the victory" (p. 191).

Belief in a supreme being undergirded Patton's almost unwavering conviction that he was destined to do great things. When his juniors got three or four stars before he did, when he was in danger of losing the chance to fight—he loved fighting—because of having slapped a hospitalized enlisted man, he kept going only because of the conviction that God still wanted him to perform a great mission. Once the war was over he knew that his luck had run out and his usefulness was finished.

He was right. Before he died on December 23, 1945, his worst qualities surfaced. Racism showed glaringly. Even though the British were white, Patton mistrusted them and felt that they had used the United States for their own selfish purposes. To him the Russians were Mongols, "a scurvy race, and simply savage" (p. 712). He believed that the postwar policy of the United States was dictated by a conspiracy of Jews, Communists, international bankers, and labor leaders, whose real aim was to destroy the United States.

War was his medium, and Patton thrived in it. There was no better combat commander in World War II. His toughness was an advantage to him in war, but he had suppressed the scholarly and poetic sides of his nature to achieve it. The scholarly emerged in the astonishing memory he had of military history. Patton was often witty and always a romantic. He received more adulation from the public, his wife, and himself than is the lot of most men, and he soaked himself in it. In sum Patton was a complex human being who is presented virtually entire and lifelike in this remarkable book.

JOHN K. MAHON
*University of Florida,
Gainesville*

H. ESSAME. *Patton: A Study in Command*. New York: Charles Scribner's Sons. 1974. Pp. x, 280. \$8.95.

CHARLES B. MACDONALD. *The Last Offensive*. (United States Army in World War II: The European Theater of Operations.) Washington: Office of the Chief of Military History, United States Army. 1973. Pp. xvii, 532, 18 maps. \$15.20.

Although both these books deal with military operations in Europe during World War II, they are quite different in format, in time frame, scope, and emphasis. Essame's book focuses its attention upon Patton as a military commander. The first two chapters provide background information relating particularly to Patton's military training and experience prior to World War II. Patton's association with the cavalry and his early interest in, study of, and experience with tank warfare receive emphasis. Thereafter, the book treats, in some detail, Patton's activities as a military commander during World War II. During the training era in the United States, he commanded the Second Armored Division and the First Armored Corps. His first operational command was the U.S. Task Force that invaded North Africa in November 1942. After the Second Corps had experienced some difficulties he briefly commanded it in Tunisia. Then followed his command of the Seventh U.S. Army in the successful invasion and conquest of Sicily. It was during the course of this operation that the slapping incidents occurred which threatened to end Patton's career as a military commander. Yet, in less than a year he appeared in France as the commanding general of the Third U.S. Army, the army that he successfully led across France and Germany, that contributed so significantly to the surrender of Germany, and the leadership of which greatly magnified his image as a military commander. With the war over Patton was unable to function successfully within the occupational command structure so he was given command of the Fifteenth Army, the unit charged with compiling the record of military operations since D-Day. A few months later, in December, he died as a result of injuries received in an automobile accident, a tragic ending for a man who preferred a soldier's death.

The Last Offensive is one of the volumes in the United States Army in World War II series and is the ninth to be published in the subseries the European Theater of Operations. In keeping with the pattern established in the earlier volumes, this book is a carefully written and detailed account of the military operations along the western front from January 1945 until the surrender of Germany in May 1945. The action of the military units is followed on the division level and, in special instances, on a lower unit level as various military operations, normally in relation to area or objec-

tives, are treated. Selective examples of such operations are the Ardennes, the Roer River dams, the Saar-Palatinate, the Rhine River crossings, and the Ruhr.

In both of these volumes Patton as a military leader is treated sympathetically. Yet, in my judgment, the evaluation of the general by Essame borders on glorification. Essame disregards completely the disastrous Hammelburg mission that Patton approved, and his treatment of the slapping incidents appears to be an apologia for the general. Both books view Patton as a highly capable but nationalistic army field commander who was a stickler for discipline and highly conscious of showmanship and personal image. They also reveal Patton's shortcomings in the international military, political, and diplomatic realm.

Patton: A Study in Command, written by a general officer with considerable experience, should have a broad appeal. It is interesting and generally well written. Although it lists a respectable bibliography, the book has no footnotes or documentation. Of special interest are the comparisons and contrasts of Patton and Montgomery.

The Last Offensive will appeal to a more restricted group of readers—army officers, military historians, and veterans of units referred to in the military operations. The book is excellently documented, with its contents being drawn from many sources. Several features deserve mention: careful and orderly organization and presentation, especially fine maps, a variety of photographs, personalization introduced via footnotes relating to the awarding of Medals of Honor, helpful appendixes, and a comprehensive index. This book compares favorably with the earlier fine volumes of the series.

DELLO G. DAYTON
Weber State College

ROBERT A. GARSON. *The Democratic Party and the Politics of Sectionalism, 1941-1948*. (London School of Economics and Political Science.) Baton Rouge: Louisiana State University Press. 1974. Pp. xiii, 353. \$12.95.

This objective, extensively researched study traces the growth of Southern discontent within the Democratic party from the unity of the early New Deal to the Dixiecrat rebellion of 1948. The author, an English scholar who teaches at the University of Keele, asserts that the Southern disaffection reflected the section's sense of its progressively declining influence within the party. Dominant in the days of Woodrow Wilson, the South became a peripheral force in the Northern, urban-based Democracy of Roosevelt and Truman. Garson finds several themes in the Southern insurgency—among them, states' rights conservatism, faith in

private enterprise, and a general belief in the region's traditional way of life—but he argues that the revolt took its major impetus from the threats to white supremacy in the policies pursued by national Democratic administrations in the 1940s. Using a term coined by V. O. Key, Garson asserts that the vote of 1948 represented a "critical election" that began a long-term pattern of Southern independence in presidential contests.

Any survey contains some omissions and debatable propositions; this one is no exception. Jesse Jones appears in neither the book's account of Texas Democratic conservatism nor its discussion of the South's role in national politics, although he was important in both. The author moves well beyond the evidence to proclaim Harry S. Truman's most private opinions on the substance and tactics of racial issues with much more confidence than I could muster. For example, there is simply no solid support for the assertion that the report of the president's civil rights committee "was far bolder than Truman had expected" (p. 233).

This book is more successful as an account of events than as an explanation of them. Its efforts at analysis are weak and limited. An important chink in its otherwise thorough research is its failure to exploit the extensive political science literature that touches on its topic. Garson cites V. O. Key's monumental *Southern Politics in State and Nation* (1949) and some other political science studies, but he fails to make effective use of them. By providing a well-organized and decently written, if somewhat unimaginative, survey of an important theme in recent American political history, Garson makes a significant contribution. But historians seeking the whys of Southern congressional and electoral behavior must still consult Key and those who have followed the path he laid out.

ALONZO L. HAMBY
Ohio University

EDWARD W. CHESTER. *Clash of Titans: Africa and U.S. Foreign Policy*. Maryknoll, N.Y.: Orbis Books. 1974. Pp. ix, 316. \$12.95.

I finished reading this book on December 19, 1974, in revolutionary Ethiopia. The U.S. Embassy in Addis Ababa, while interested in Ethiopia's future, was more concerned about its budgetary chances for the next fiscal year and was therefore taking no action that would jeopardize its public relations stance before Congress. Embassy officers talked about the status quo ante in glowing, nostalgic terms; the image of a wise, old emperor—the very one who stood before the League of Nations to warn the world about the future—seemed immeasurably better to them than the specter of a sharply leftist Ethiopia, perhaps even in cooperation with Mao's myrmidons. To me it seemed as if official

America again was playing ostrich about the real needs of a terribly poor country that needed a sharp break with the past in order to progress toward modernization. Oddly enough, through inadvertence and sheer analytical misconstruction, Dr. Chester's book clearly defines the status quo nature of American foreign policies toward African countries and Washington's almost negligent misreading of the continent's needs.

If I am mystified by the title of the book—just look up "titan" in any dictionary—then I am bewildered by the author's decision to write such a study. Not a trained Africanist, he is bound by data derived primarily from the more obvious secondary works—this approach is also for the American side. In any case he is sublimely unaware of those facts that are the intimate stock in trade of the Africanist. He does not know, for example, that the American Society for African Culture was funded, in part, by the CIA. He does not realize that there were private U.S. loans to Liberia before the famous Firestone transaction of 1925. He shows little understanding of Ethiopian foreign policy toward the United States, and, for that matter, he has great difficulty clarifying U.S. foreign policy objectives in Ethiopia and the Horn of Africa because he fails to make his analysis within the idea of the Red Sea-Indian Ocean-Persian Gulf strategy. Worst of all, Chester has not fully exploited the many M.A. and Ph.D. theses—there are at least ten at Howard University, for example—available on the subject of U.S.-African relations.

These criticisms, which may derive from my specialized knowledge or my biases, do not, by themselves, totally discredit this book. The author himself accomplishes this act of destruction by the very shallowness of his analyses. He informs us that there is no uniform U.S. policy in Africa and that in treatment there are many country-by-country disparities. He overwhelms us by claiming that Washington will remain concerned about Africa "at least to a limited degree" because of U.S. need for raw materials and its fear of a Communist takeover. If these tautologies were not enough, Chester strikes a blow for understanding by arguing very seriously that "the average white American experiences difficulty in identifying with a continent whose goals and aspirations are somewhat different from those in the United States"—very heady stuff indeed! The triviality of such analyses unfortunately matches the trivial nature of U.S.-African relations. Chester has written a book whose time has not come.

HAROLD G. MARCUS
Michigan State University

LYNN H. MILLER and RONALD W. PRUESSEN, editors.
Reflections on the Cold War: A Quarter Century of Ameri-

can Foreign Policy. Philadelphia: Temple University Press, 1974. Pp. viii, 207. \$10.00.

RAYMOND ARON. *The Imperial Republic: The United States and the World, 1945-1973*. Translated by FRANK JELLINEK. Englewood Cliffs, N.J.: Prentice-Hall, 1974. Pp. xxxviii, 339. \$10.00.

The essays edited by Miller and Pruessen give the reflections of a number of historians, political scientists, and others, most of whom are well-known revisionists. They are worth reading for their interpretive content, but the reader should not expect to gain any significant new information. Miller's own essay, dealing with the United Nations, argues that the United States has sought to build up and use the UN primarily when it was in its self-interest to do so. He notes that the place of the UN in American policy reached its lowest point with the Nixon administration. Pruessen's essay focuses on 1947 and concludes that non-cold-war factors, namely economic considerations, were predominant in the determination of United States policy in that year. The reader is, however, left in doubt as to the exact nature of those economic considerations. Harrison E. Salisbury, writing on the origins of the cold war in Asia, speculates that the Russian move in Korea in 1950 was directed more against China than the United States, and that the Sino-Soviet split might have occurred in 1950 had the United States not driven the two communist rivals together. Walter LaFeber's selection, "Crossing the 38th," Barton J. Bernstein's "The Cuban Missile Crisis," and Richard J. Barnet's "The Cold War and the Arms Race" go over mostly familiar ground. Richard A. Falk's essay, "Counterrevolution in the Modern World," is the most strident of the revisionist selections. He asserts that the counterrevolutionary posture of the United States developed into a counter-insurgency mission with genocidal tactics and effects. "Revision Revisited," by Stanley Hoffman, and "The Asian Boundaries of Coexistence," by Norman B. Hannah, are much less revisionist in tone than the other essays. Hoffman attributes the beginning of the cold war to misperceptions on both sides. Hannah, while obviously favoring co-existence, observes that coexistence is not the opposite of cold war. It is, rather, the fruit of successfully waged cold war.

Raymond Aron's prize-winning volume, which has been much acclaimed in Europe, is a rebuttal to "para-Marxists" such as Claude Julien. William Appleman Williams, and Gabriel Kolko. Aron, a professor at the College of France, has written more than a dozen books. This one, as was said by *Le Nouvel Observateur*, has "rigor of thought combined with clarity of expression," but it is not a work of original historical research. Like the essays reviewed above, it is valuable chiefly for its inter-

pretive content. Aron argues that in saddling the United States with causal responsibility for the cold war the revisionists have succumbed to the national myth of American omnipotence. He defends the policy of the Truman administration right down the line—not so the Eisenhower administration. The inflexibility of Dulles's diplomacy he regards as wide open to criticism. As for the later period, he gives high marks to Kennedy in the Cuban missile crisis, and to Nixon and Kissinger in the Vietnam negotiations. Fully half of Aron's book is devoted to a discussion of the United States in the world market. Though he agrees with revisionists on some points, he rejects outright the theory that American diplomacy since 1945 has been governed by the American economy's place in the world market.

Since neither of the two books under review present important new evidence, it is doubtful that either will bring a noticeable shift in the battle lines between the revisionists and the nonrevisionists.

RAYMOND A. ESTIUS
Tulane University

Foreign Relations of the United States, 1948. Volume 4, *Eastern Europe; The Soviet Union*. (Department of State Publication 8743.) Washington: Government Printing Office, 1974. Pp. xv, 1161. \$10.80.

These documents cover the period of intense cold war, the period of respective American and Soviet containment policies. (Documents related to the Berlin blockade are not included.) The civil war in Greece was still going on, while Eastern Europe was in that year fully integrated within the Soviet zone of influence.

The Truman Doctrine of 1947 led the National Security Council to declare on January 6, 1948, "The security of the Eastern Mediterranean and of the Middle East is vital to the security of the United States" (p. 2). The United States in its new quality of a Mediterranean power offered support to the Greek government, which was engaged in a protracted struggle against the Communist insurgents who were helped in turn by the Soviet bloc, including Yugoslavia. Yugoslavia ended its assistance only in 1949 following its quarrel with the Cominform. It is interesting for a better understanding of the present American policy toward Cyprus to read the opinion by the Joint Chiefs of Staff in 1948: "Turkey is strategically more important than Greece" (p. 191). The United States adopted a cautious but hopeful attitude toward Yugoslavia after the Yugoslav excommunication by the Cominform (pp. 1054-1118).

The documents related to the Belgrade Conference on the regime of navigation on the Danube River (pp. 593-732) demonstrate the nature of So-

viet policy in a situation where the USSR was able to muster the majority of votes. The Soviet draft convention was adopted by the majority vote of the Soviet bloc over the vain protests of the United States, Britain, and France. The new convention prohibited the use of the river by the nonriparian vessels (p. 647) and restricted the composition of the Danubian Commission to the riparian states, including the USSR. The United States, Britain, and France were excluded (p. 646).

The most dramatic event of 1948 was the Communist coup in Czechoslovakia. Here as elsewhere in Eastern Europe the United States could only register its protest (pp. 733-58). In spite of the Berlin blockade Secretary of State George Marshall thought that "the Kremlin does not intend to mount any action in Europe proper which would carry the risk of actual hostilities" (p. 840). The British secretary of state, Ernest Bevin, shared this view (p. 844). Dispatches from the American embassy in Moscow noted Stalin's policy of tightening the isolation of Soviet population from all Western contacts (pp. 890-96).

The usual American reaction to the events in Eastern Europe remained the same regarding the Romanian, Hungarian, and Bulgarian violations with the Soviet approval of the 1947 peace treaties. It consisted in dispatching protest notes (pp. 279-395). The United States, however, reinforced in 1948 its restrictions on trade and in the civil aviation relations with the USSR and Eastern Europe (pp. 436-592).

The exception was made for Finland once it became clear that Stalin was ready to grant that country a treatment more favorable than that given the rest of Eastern Europe. The State Department correctly attributed Stalin's leniency to his fear that a harsher treatment of Finland would have had pro-Western repercussions among the Scandinavian states, in particular Sweden (pp. 780, 785). The other documents in this volume relate to such controversial issues as the lend-lease settlement (pp. 950-1023) and the fate of teachers at the Soviet school in New York who sought refuge in the United States (pp. 1024-53).

W. W. KULSKI
Duke University

IRWIN UNGER, with the assistance of DEBI UNGER.
The Movement: A History of the American New Left, 1959-1972. New York: Dodd, Mead and Company, 1974. Pp. viii, 217. Cloth \$5.95, paper \$3.75.

This concise, comprehensive volume is apparently intended for a general audience and for use in the classroom, but it is a pleasant surprise to one who has in mind the interests of *AHR* readers. Unger has obviously digested a long shelf of relevant liter-

ature, and he utilizes the holdings of the Social Protest Project, Bancroft Library, Berkeley—especially valuable since in this instance Richard Armour's doggerel applies: "So leap with joy, be blithe and gay, / Or weep, my friends, with sorrow. / What California is today, / The rest will be tomorrow."

Unger discusses the origins of "the movement" among young intellectuals, to whom Mills gave a role and Marcuse a rationale, and the reasons for its spread among the children of the affluent in the universities. What follows is a tight, well-written narrative of organizational and doctrinal developments on the road "from reform to revolution" (Edward J. Bacciocco's subtitle) or "from Port Huron to Peking" (John P. Diggins's phrase). Regarding the radicals' motives, Unger is judicious and precise in weighing environmental factors (society's ills) and psychological factors (the protesters' personal problems). He is also perceptive on the "tangled interconnection" between cultural and political dissent. Factionalism, Unger believes, had far more to do with the movement's failure than repression, but the main difficulty was that the New Left "had left the rest of America so far behind that it had lost contact with the national reality." Foreign policy was another story. "I think it [the New Left] forced the United States out of Vietnam," Unger declares, while citing the military prowess of the Viet Cong and the North Vietnamese as an indispensable element in this result and pointing out that there was a responsive chord to be struck in the form of growing popular disillusionment with the Vietnam War.

In *Radical Paradoxes* (1973) Peter Clecak, who, unlike Unger, is a veteran of the movement, concludes that the present order can "not be changed as a whole all at once." One who is interested in such judgments as well as the basic facts about the New Left will find Unger's book an excellent introduction to the subject.

BERNARD STERNESHER
Bowling Green State University

Public Papers of the Presidents of the United States. Richard Nixon. Containing the Public Messages, Speeches, and Statements of the President. [Volume 4.] 1972. (Office of the Federal Register, National Archives and Records Service, General Services Administration.) Washington: Government Printing Office, 1974. Pp. xlix, 1161, 28, 21, 7, 3, 25, 2, 80. \$18.55.

This volume of the *Public Papers* is particularly fascinating, as it captures well the developing campaign strategy and diplomatic ploys of an administration bent on retaining political power at home while promoting stability and détente abroad. Needless to say, many of the proposals and prob-

lems found in the earlier volumes of this series are carried over into 1972: Vietnam, the state of the economy, racial politics, the "New Federalism," executive reorganization, environmentalism, welfare reform, and the quest for détente are once more on the presidential agenda. And each of these issues is worthy of extended comment.

But in a brief review only items of major importance can be mentioned. Of particular significance for students of the Nixon presidency are the State of the Union Address (pp. 34-41) and the accompanying State of the Union message (pp. 41-74) that spell out the administration's program for the new year—an election year. And not to be overlooked is the comprehensive statement on the subject of American foreign policy that the White House submitted to Congress (pp. 194-346). It is an important document, since it provides a philosophical base and operational guide to what the administration called the "New American Foreign Policy" (p. 196). Material covering the president's trip to China—which Nixon felt was "the week that changed the world" (p. 379)—is prominently featured, as is his later visit to Moscow, where accords were signed further to advance détente (pp. 633-44).

The central issue in the realm of foreign policy still remained Vietnam. On that subject Nixon made it clear, time and again, that American honor would be preserved and American interests protected. His speech on May 8, announcing the mining of Haiphong harbor and the continuation of the bombing of North Vietnam, could be viewed as an act of desperation or a shrewd and ruthless move to end the hostilities (pp. 583-87).

Tied to his Vietnam stance was Nixon's carefully organized campaign to achieve re-election. Attempting to establish a "new American majority," by unifying under fresh leadership what were once major elements of the Roosevelt coalition, he celebrated the work ethic, attacked quotas, evangelized on the subject of patriotism, supported the neighborhood school system, opposed busing, offered help to parochial education, and endorsed efforts to stimulate the development of "black capitalism." Furthermore, whenever appropriate, he reminded his audiences that the economy was booming and that inflation, thanks to his policy initiatives, was very much under control (p. 1059).

Thus garbed in his Peking pinstripe, the mandarin president awaited the election returns with keen anticipation; as predicted, the landslide developed to vindicate what he called the "American system" (p. 789). So 1972 was indeed Richard Nixon's apotheosis; his year of glory and redemption had given him the opportunity to serve another four-year term as president of the United States.

Like its predecessors in this series, volume 4 is superbly edited, handsomely printed, and is accompanied by a well-organized appendix that provides additional information, trivial or otherwise.

WILLIAM C. BERMAN
University of Toronto

CANADA

H. V. NELLES. *The Politics of Development: Forest, Mines & Hydro-Electric Power in Ontario, 1849-1941*. [Hamden, Conn.: Archon Books, 1974. Pp. xiii, 514. \$21.00.]

This volume is more original than might be suggested by the author's assertion that his work belongs in the Innis-Lower-Creighton tradition of studies of the political economy of staple production in Canada. It does indeed belong there, and worthily, but at the same time it opens up new territory. While Nelles disclaims any attempt to write a full-scale economic and political history of Ontario or the business history of any industry, his thoroughly researched and well-written examination of the interplay between government and business in the development of the forest and mineral staples and hydroelectric power that has dominated the economy and politics of Ontario in the twentieth century is a major contribution to the neglected history of the wealthiest, most industrialized, and most populous province of Canada.

The early Crown ownership of natural resources in preindustrial Upper Canada formed a firm basis for a concept of the positive, interventionist state in Ontario and for proprietary control over the three most important natural resources of the province. Yet the book describes the growth of relationships between the state and business very like those discerned by Gabriel Kolko, James Weinstein, Ralph Miliband, and others in the United States where natural resources were early handed over to the control of private interests. Although public ownership of the production and distribution of hydroelectric power in Ontario represents a deviation from "the North American norm," the author considers the influence of Ontario Hydro negligible in affecting the general pattern of industrial capitalism.

Challenging the view that in Canada state intervention owed much to a "red Tory" tradition, Nelles contends that in Ontario as in the United States business interests cooperated actively with the state because they needed the power of government to further their projects for resource development and were never seriously restrained by attempts on the part of government to put public over private interest. It is difficult to refute Nelles's

conclusion that the result was a severe limitation on the practice of responsible government in Ontario. It is to be hoped that this conclusion will be put to the test of comparison with equally impressive studies of the politics of resource development in other Canadian provinces.

MARGARET PRANG

University of British Columbia

GENEVIÈVE LALOUX-JAIN. *Les manuels d'histoire du Canada au Québec et en Ontario (de 1867 à 1914)*. (Histoire et sociologie de la culture, 6) Québec: Presses de l'Université Laval; distrib. by International Scholarly Book Services, Portland, Ore. 1974. Pp. 250. \$10.00.

For several years in the decade of the sixties, Canadian history, historians, and social scientists drank deeply of the milch cow known as "The B&B Commission," the Royal Commission of Inquiry on Bilingualism and Biculturalism. It was established by the Pearson government in reaction to the quiet and not-so-quiet Quebec Revolution that unleashed divisive forces within the Canadian Confederation. One of the studies undertaken for the B&B, by Marcel Trudel and Geneviève Laloux-Jain, concerned history textbooks used in Canadian schools. Laloux-Jain undertook, as a Ph.D. thesis, a narrower study of those texts used in Quebec and Ontario schools. Her results are now presented in a series devoted to the History and Sociology of Culture (of Quebec) published by Laval University.

The author has used a very traditional approach, a theory of nationalism—which is not a theory but a straight and somewhat shallow survey of a few books on nationalism. (The very title of her book is somewhat misleading. Although not mentioned in the title, the basic theme of the book is the teaching of nationalist sentiment by means of history.) Laloux-Jain goes on to present "official programmes" and the "cultivation of national sentiment" in the two provinces. This is followed by the criteria used to select the history books imposed on the teachers and pupils. Well over half-way through the book, selections from history books on specific events and their interpretations are presented.

The author, in her conclusion, informs us that it is generally accepted that history textbooks are directly responsible for an increase in nationalist sentiment and that the books used in Ontario and Quebec indicate a close relationship between the aims of the individuals in charge of the programs and nationalist objectives, although the latter was "neither the *unique* nor *predominant* objective" (italics added).

There are any number of ways that a study such as Mrs. Laloux-Jain's can be undertaken. The one

chosen and imposed, that is, history and nationalist sentiment, while valid as part of a study concerned with the inquiry of the B&B, is less useful as an analytical tool in a series devoted to the History and Sociology of Culture—whatever that means. The author indicates that words and concepts such as civics, ethics, morality, and patriotism, among others, as well as concepts of nationalism were encountered in her study. It might have been more fruitful to use word and content analysis to establish their weight and role in the books she has examined rather than to be led to an unsatisfactory conclusion on the basis of the evidence presented.

CAMERON NISH

Sir George Williams University

TERRY COPP. *The Anatomy of Poverty: The Condition of the Working Class in Montreal 1897-1929*. (The Canadian Social History Series.) [Toronto:] McClelland and Stewart. 1974. Pp. 192. \$3.95.

Until recently Canadian historiography has been dominated by economic and political history. Professor Copp's volume is one of a series of in-depth studies of social history designed ultimately to portray "the context in which Canadians have lived and interacted with one another." As such it rests upon and includes significant amounts of quantitative data, designed to illustrate and to account for the economic and social conditions of the working class of Montreal between 1897 and 1929.

In general the first three decades of the twentieth century were a period of sustained growth in the Canadian economy. Montreal, Canada's largest city, shared in this development, but the benefits did not accrue to its wage workers, who constituted two-thirds of the population of the city. Their position was and continued to be precarious in the extreme. Low wages and frequent unemployment were accompanied by a high incidence of child labor, poor housing, inadequate schools, and high mortality rates. If revolutions were born of misery, Montreal should have been a prime candidate for social upheaval throughout most of the period examined by Copp.

More important, perhaps, is the author's explanation of the phenomena he has described. He contends that the source of the problem was not the "alleged cultural differences" between Quebec and English-speaking Canada. Montreal was a city like others in America, exposed to the impact of a free-market economy and in which the workers paid "the price for industrial growth." Conditions in Montreal were relatively worse because of locational factors, the structure rather than the ideology of French-Canadian society, and the failure of the city to develop a tax system adequate to its needs.

This is a brief but carefully written study based upon a solid knowledge of the relevant materials. It throws an important sector of the social history of Canada into harsh and comprehensible relief. It should stimulate further investigations into the social effects of industrialization in Quebec and comparative studies of those effects in Quebec and other parts of Canada.

G. S. FRENCH
Victoria University

ANDRÉ-J. BÉLANGER. *L'apolitisme des idéologies québécoises: Le grand tournant de 1934-1936*. (Histoire et sociologie de la culture, 7.) Quebec: Presses de l'Université Laval; distrib. by International Scholarly Book Services, Portland, Ore. 1974. Pp. 392. \$13.00.

The era of the Great Depression has been of great interest to Canadian historians in recent years. Among outstanding contributions are the general overview of Blair Neatby, *The Politics of Chaos* (1972) and Michiel Horn's collection of documents and essays, *The Dirty Thirties* (1972). A significant reason for this interest is stated by Neatby, who claims that the attention of thoughtful observers was shifting "from the frontier to the metropolis, from the challenges of geography to the tensions between regions, classes and cultures. It would have been a period of controversy and confrontation even without an economic crisis." The obvious analogies between the economic problems of the thirties and those of the present era heighten interest even further.

Bélanger's study is a preliminary report on this period as viewed from Quebec. In successive chapters he analyzes and appraises various individuals, groups, and publications that represent the views of the intellectual elite of Quebec. In his introduction Bélanger strives to place his subject into the broader context of North American and European political "turning points," but the main body of his work more often demonstrates the intellectual isolationism of French-Canadian thought from the rest of Canada along the classic lines once set forth by Michel Brunet: "agriculturalism, messianism and anti-statism," the last noted comparable to the apoliticism of Bélanger's title. The consistency of these themes demonstrates to the author the validity of Louis Hartz's characterization of Quebec society as a feudal fragment of Europe.

The first half of the book is different from the second in both approach and subject matter. Here the author deals with the influential Montreal newspaper, *Le Devoir*. This section contains quantitative analyses of editorial content as well as the author's summaries of and commentaries on the leading writers of the paper. He describes a portion of articulate Quebec opinion that at times

related to the problems of the depression in an industrialized, urbanized society with significant agricultural, mining, hydroelectric, and other sectors. Like many other Canadians, these writers may not have had many answers to the problems of the times but they chose to grapple with some of the questions.

By contrast, in the rest of the book the author turns to the more generally consistent traditional expressions of Quebec thought. The most important single individual in this section is Abbé Lionel Groulx. Here the apolitical and antipolitical viewpoints are reflected more often, the *mystique de la terre* emphasized as well as the messianism of Groulx and others. One of Groulx's particular talents was his promotion of historical heroes of the French-Canadian past. His models for a heroic leader for his own time and place were Salazar of Portugal, Dollfuss of Austria (while he lasted), and others of the *type mussolinien*.

The leader Quebec got was Premier Maurice Duplessis, who rose to power after two significant election campaigns—in 1935 and 1936—in which the press gave him much indirect aid by publicizing the corruption of the ruling Liberal regime. These less transcendental developments return us to the first section of Bélanger's book. Actually, he has written two valuable studies within the binding of a single book.

JOSEPH A. BOUDREAU
San José State University

LATIN AMERICA

FREDERICK P. BOWSER. *The African Slave in Colonial Peru, 1524-1650*. Stanford: Stanford University Press. 1974. Pp. xiv, 439. \$16.50.

Bowser has given us the most complete, reliable, and informative book yet to appear on blacks in any Latin American country during the colonial period, and that includes Brazil. In a sense, he tells us all we need to know about the role of blacks in Spanish America, or in any Latin American situation where the sugar industry was not the base of the economy. The special attributes of the black population of the early period in Peru—intensive or skilled work, proximity to Europeans, an intermediate or auxiliary function in the Spanish-Indian context, a slow absorption into the lower levels of Spanish society—extend forward unchanged to 1650, more than enough time to demonstrate that we are dealing with Hispanic American universals. The author covers his topic in many different aspects, from the slave trade to hard statistics on prices and population to governmental-ecclesiastical action and policy debates; but at the core of the book are the impressive chapters on the patterns of activity of the blacks

themselves, both slave and free—patterns taken from an overflowing wealth of individual lives and cases. Bowser demonstrates that it is possible, for colonial Latin America, to do the social history of a special group over a long time span without sacrificing the depth and subtlety of insight that come from large-scale use of fresh individual examples. This single book contains more lives of blacks, more organized, re-created reality from the world of the Latin American black, than the rest of the historical literature put together. The accomplishment is all the more admirable because blacks were such a diversified group, attached to every sector of the European population.

The study includes both social and institutional history. If the space devoted to the organization of the constabulary or the hysterics of official correspondence tends to elevate these matters wrongly to the same level of importance as the social-economic functioning of a major population group, nonetheless Bowser ultimately reinterprets or discounts the utterances of officials and interest groups in the light of the social reality he has so deeply studied, with a large net gain for knowledge. In one case this is not possible. The borders of modern Peru, on whose coast the black population was concentrated, set the geographical framework of the study, so that the great highland silver mines of Potosí fall outside it. Thus the Peruvian debates over Indian versus African labor at the mines are reported in a vacuum. As a corrective, one might read Inge Wolff's substantial article on blacks at Potosí, "Negersklaverei und Negerhandel in Hochperu, 1540-1640," in *Jahrbuch für Geschichte von Staat, Wirtschaft und Gesellschaft Lateinamerikas*, 1 (1964): 157-86; an item somehow not in Bowser's bibliography. Wolff reveals the presence of a sizable minority of blacks in the Potosí area, working in refineries, crafts, domestic service, and intensive market agriculture; she thereby handily confirms and extends Bowser's main analysis and shows how far removed the labor controversies in Lima were from the demographic and economic realities. The latter, in conjunction with established Hispanic social practices, had already determined that in the Andean silver mines as elsewhere, blacks would be a skilled minority rather than the main labor force.

The riches of Bowser's study deserve many hours of a reader's attention and many pages of a reviewer's comments.

JAMES LOCKHART
University of California,
Los Angeles

DONALD J. MABRY. *Mexico's Acción Nacional: A Catholic Alternative to Revolution*. [Syracuse:] Syracuse University Press. 1973. Pp. xiv, 269. \$15.00.

In re-creating the origins and development of Mexico's most important opposition party, the Acción Nacional (PAN), Donald Mabry has organized his narrative into two parts. The first is a chronological and political history of PAN from its beginnings in the 1930s to the Echeverría administration of the 1970s. The second section is topical and contains most of the socioeconomic, electoral, and budgetary data from which Mabry generalizes about PAN's doctrines, structure, leadership, and membership. One problem resulting from this organization is that the historical section has been reduced to a narrow electoral history with very little substance and too many repetitious details, such as the never-ending examples of election fraud in Sonora, Baja California, and the Yucatan. (The latter is more a criticism of the editors than of the author.) In any case, the study taken as a whole is a worthwhile and scholarly introduction to the topic, worthwhile in its pervasive thesis and scholarly in its thorough research—for instance, Mabry had access to secret party files rarely seen by outsiders.

Mabry's thesis, amply demonstrated, is that PAN evolved from a conservative Catholic lay movement, led by wealthy industrialists and bankers who opposed the socialism of Cardenas's educational reforms in the 1930s, to a secular political party, led by urban-based professionals, intellectuals, and businessmen who, following the Christian Democratic model, sought to apply to Mexican society a social reform model derived from Catholicism. As the Party of the Institutionalized Revolution (PRI) moved to the right after 1946, PAN evolved from a right-center coalition to a left-center party of opposition. While PAN's counterparts in Latin America are readily recognizable as Christian Democratic, the peculiar traditions of anticlericalism in Mexico along with PRI's monopoly on all revolutionary jargon has left PAN with a conservative, clerical, and counter-revolutionary image that it does not deserve.

Mabry has no illusions about the limits of PAN's power in what is essentially a one-party system. Although not exactly a "kept" party, PAN exists and succeeds only at the pleasure of the Revolutionary Family, an oligarchy that prefers to project the image of a liberal, democratic state. Lack of patronage, limited finances, fraudulent elections, manipulation of the party deputy system by PRI authorities—all of these factors prevent PAN from not only being a Catholic alternative to revolution, but, even within the context of the rules of liberal, middle-class, "bourgeois" politics, a reasonable alternative to the Party of the Revolution.

WILLIAM DIRK RAAT
State University of New York,
College at Fredonia

Communications

A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.

TO THE EDITOR:

In reviewing my book, *Art and Politics: Cartoonists of the Masses and Liberator* (*AHR*, 80 [1975]: 521-22), Professor Louis Filler has little to say except that I tend to agree with the artists' critical view of established social values, while he believes "that the basic values of society [are] right." That scarcely fulfills a reviewer's obligation to say something illuminating about a book and its subject matter, rather than wishing it were another book on a different subject. Beyond that, his snide remark about my acknowledging assistance from people who are employed by certain privately endowed institutions is pointless. Where was I supposed to obtain materials and suggestions, from transcendental beings? We all have to live in the world that is, whatever attitude we take toward it, critical or celebratory.

I only wish to express my disappointment that my reviewer's ideological commitments prevented him from coming to grips with the book I did write.

RICHARD FITZGERALD
Laney College,
Oakland, California

PROFESSOR FILLER REPLIES:

One of Mr. Fitzgerald's problems as a historian is that he does not understand the role of irony in scholarly debate. It is ironical that sweeping revolutionary statements can emanate not out of revolutionary milieus, but conventional ones. I do not say this is bad—it is cause for pride—but it is worth noting among scholars. Irony is related to art and even to scholarly statement. Fitzgerald's interest in his artists' "critical view of established social values" can use broader frameworks. He can remark, in my forthcoming "Appointment at Armageddon," the hard political point Robert Minor made as a patriotic American in 1912 before he joined his great crusade. It might inform Fitzgerald on the dynamics of Minor's art, before as well as after his political commitments turned to stone.

The reader will be interested to note, if he happens not to have read my review, that I did not say I believed "that the basic values of society [are] right." I quote my relevant sentence: "The author is not aware that there are those who think, or thought, that the basic values of society were right, and that to demonstrate their invalidity, and the proved rightness of his alternative values requires greater subtlety and a sounder grasp of artistic and human principles than is here indicated" (p. 522). Although I would not put this sentence beside the better ones of T. B. Macaulay, I still suggest it merits Fitzgerald's thoughtful consideration.

LOUIS FILLER
Antioch College

TO THE EDITOR:

Professor Stromberg's *étude*, "Some Models Used by Intellectual Historians," was very pleasant reading and a useful composition of contrapuntal themes in the history of ideas. Two sentences toward the end affirm his view of the historian's role as a bringer of order out of chaos. "We can proceed to a more complex historiography, which is not necessarily a chaotic one"; and, "It is up to

historians to show that we can restore order" (*AHR*, 80 [1975]: 573). He neglects the view that reality, including the history of ideas, may, *au fond*, be chaotic, and that our abstractions may be but attempts to make that frightening prospect tolerable. This casts the historian, and others who deal with ideas and perceptions of the condition of man, in a rather more religious role as reconcilers of man to reality.

WILLIAM W. MOSS
John F. Kennedy Library

TO THE EDITOR:

The review by Carleton S. Coon of Cyrus Gordon's *Riddles in History* (*AHR*, 80 [1975]: 610-11), contains a number of errors. The reviewer states, "The use of cryptograms (riddles) goes back to ca. 700 B.C., was lost during the Middle Ages, and rediscovered in 1969." Yet Alf Mongé, to whom Gordon's book is dedicated, claimed in *Norse Medieval Cryptography in Runic Carvings* (1967) that he had found many. In a paper published in *Minnesota History* (41 [1968]: 34-42) Aslak Liestøl, one of Norway's leading authorities on runes, wrote that cryptograms are certainly not unknown in runic writings and that even runologists have been aware of the fact for centuries. The review accepts Gordon's claim that he and Mongé have authenticated four texts generally called forgeries. One of these is the "Kensington Stele," better known as the Kensington rune stone. Every expert runologist in Norway, Sweden, Denmark, and the United States agrees that the inscription cannot be from the fourteenth century but is, in the words of Konstantin Reichardt, "a genuine rune stone, to be sure, but a clever work of the late nineteenth century." (For a summary of the opinions of runic experts, see my article, "The Kensington Rune Stone" [*Minnesota Archaeologist*, 26 (1965): 97-115]). The testimony about the finding of the inscribed stone is uncertain and often contradictory. In *Norse Medieval Cryptography in Runic Carvings*, in cooperation with Ole Landsverk, Mongé tried to show that the Kensington inscription should not be read literally but is really a cryptogram. This attempt, as Abram Sinkov, an acknowledged expert in cryptography, wrote in a letter to me, was demolished by Aslak Liestøl in his article in *Minnesota History*. I have seen an exchange of letters between Mongé and Sinkov in which the latter explains the defects in Mongé's procedure. The finder of the stone, Olaf Ohman, may have been "almost illiterate" in English, but as Theodore C. Blegen, in his book on the rune stone, and others have established, he was not illiterate in Swedish. He knew something of runes and one of his close friends, Sven Fogelblad, knew a great deal about them.

It is unfortunate that the reviewer of a book that includes discussion of the Kensington rune stone is apparently not acquainted with the state of that question.

LAWRENCE D. STEEFEL
University of Minnesota

DR. COON REPLIES:

My answer is: I stand by my guns.

CARLETON S. COON
Gloucester, Massachusetts

TO THE EDITOR:

Professor Orlow's review of my book, *Mittelstand, Demokratie und Nationalsozialismus* (*AHR*, 80 [1975]: 675-77), is very flattering. There are a few judgments, however, that I would like to contradict. I am not of the opinion that "the *Mittelstand* in the twentieth century is an obsolete social grouping" that "has been objectively bypassed in the linear progress of history." What I describe as obsolete is the "social protectionism" of Imperial Germany and the mentality it reinforced within the ranks of craftsmen and shopkeepers. As far as the economic position of the *Mittelstand* is concerned, I rather stress a long-range trend toward stabilization.

The "revisionism" of my book has more than one target: I question, first, the proposition that the rightward movement of the *Mittelstand* was a more or less spontaneous reaction to the economic crash of 1929; second, the well-known interpretation of nazism as an "extremism of the center" as developed by Seymour Martin Lipset, a theory that ignores the striking continuity between the vulgar conservatism of the late nineteenth century and the Nazi ideology; third, the Marxist scheme according to which the final disappearance of small-scale production is a necessary outcome of the economic process; fourth, the vulgar Marxist identification of fascism with the politics of certain groups of monopoly capital. I therefore do not think that my approach can be properly described as part of a "neo-Marxist framework." In spite of all the merits of Marx's analysis of class conflicts and ideologies, which no historian should be allowed to ignore, I am fully aware of some of the ahistorical assumptions on which his theory of the historical process is based. I have discussed these shortcomings of Marxist analysis in my article "Zum Verhältnis von bürgerlicher und proletarischer Revolution bei Marx und Engels" (in Hans-Ulrich Wehler, ed., *Sozialgeschichte Heute. Festschrift für Hans Rosenberg* [Göttingen, 1974], 326-53). My book on the *Mittelstand* is a more

practical example of that kind of historical revisionism.

HEINRICH AUGUST WINKLER
University of Freiburg

PROFESSOR ORLOW REPLIES:

Professor Winkler and I are not really in disagreement on the merits of his book. The targets of his superb analysis are indeed multiple. At the same time, I did feel obligated to point out what seemed to be a discrepancy—if not an inconsistency—between the tone of the introductory passages of his book and the detailed analyses in the individual chapters. I am delighted to have him confirm that my impression of his working hypothesis was incorrect.

DIETRICH ORLOW
Boston University

TO THE EDITOR:

So long as a reviewer does not falsify in his description of a book, an author's response to negative commentary ought to be silence, leavened by whatever humility, stoicism, or contempt is appropriate. But Professor Howe's review of my *The New Heavens and New Earth: Political Religion in America* (*AHR*, 80 [1975]: 709–10) charges me with an unspecified derivative dependence on secondary authorities, which is quite unjustified. A close reader would have seen, for example, that while I have learned much from Richard Bushman, Alan Heimert, and Mark DeWolfe Howe, my interpretations of the Great Awakening, the Revolutionary clergy, and the Supreme Court's reading of the First Amendment's religious clauses amplify and revise their formulations through original research; forty-five out of sixty-four footnotes in my chapter on the Whig Calvinists cite primary sources. Furthermore, in linking up generational statistics with Eriksonian concepts to explore the Great Awakening, in applying sociological sect typology to the abolitionists, in comparing Black Muslims with Mormons, and in using and criticizing Tocqueville's theory of political religion, for example, my book follows in nobody's footsteps. I would welcome criticism of any of these interpretations, but Howe has ignored them all in a dismissive denigration that never comes to terms

with the fact that my book is organized "in dialogue with Tocqueville," as its rationale. Even on small matters Howe is not a scrupulous reader, accusing me of omitting the Transcendentalists, even though my index cites seven references to them. In all objectivity, not to mention gloomy irritation, is it unreasonable to expect more rigor and fair-mindedness than this from an *AHR* review?

CUSHING STROUT
Cornell University

PROFESSOR HOWE REPLIES:

My review criticized Professor Strout's latest book for lack of originality, remarking that it recapitulates the writings of others too much. The first dozen pages, for example, discuss nine secondary authorities. Even the "Calvinist Whigs" chapter, where Strout shows familiarity with the sources, is cast principally as a commentary upon Heimert. Elsewhere Strout repeats existing literature too uncritically, as in his disparaging treatment of the women's suffrage movement and progressivism in general. I also pointed out the book's erratic coverage. Why are we told about the Protestant clergy's support for World War I—via a summary of a forty-year-old monograph—but not about the churches' stands on war in 1812, 1846, or 1898? As for American Transcendentalism (no "small matter") the "seven references" to it are all in passing. Strout should either have addressed the movement directly or made clear why he did not.

Professor Strout's letter offers four examples to show his originality, but only the application of sect typology to abolitionism lends real support to his defense. The Great Awakening has been related to Eriksonian psychology by Richard Bushman (1969), Philip Greven (1972), and Gerald Moran (1973). The parallel between Mormons and Black Muslims is only briefly stated and not pursued. And how can Strout possibly claim originality for engaging in a "dialogue" with Tocqueville?

I respect Cushing Strout and feel sorry about his present mood of "gloomy irritation." His previous writings have set a high standard, by which his latest one suffers.

DANIEL WALKER HOWE
*University of California,
Los Angeles*

Recent Deaths

Professor Emeritus MARSHALL W. BALDWIN, who died July 3 in Roosevelt Hospital, New York, was a distinguished authority on medieval European history. During nearly forty years at New York University he was a steadily productive scholar, an exceptionally effective teacher, and a friend and associate whose singular modesty and sweetness of character subtly but profoundly influenced all who had the privilege of knowing him. He was born on March 30, 1903, in New Haven, Connecticut, to Charles Sears Baldwin, professor of English successively at Yale University and Barnard College, and Gratia Eaton Baldwin, an authority on Dante.

Upon graduation from Kent School, Marshall Baldwin entered Columbia College in 1920 from which he obtained his B.A. degree and election to Phi Beta Kappa in 1924. Spare and athletic, he had spent his summers boating, swimming, and winning tennis trophies. He was oarsman with a winning crew during a period of study at Cambridge University. From Princeton University he received his Ph.D. degree in 1934.

Thereafter, neither the crisis of war nor personal tragedy interrupted scholarly production. Contributions to symposia, scholarly journals, and encyclopedias were accompanied by the regular publication of major works or volumes in collaboration with other scholars, such as his coeditorship with Kenneth Setton and contribution to the five-volume *Pennsylvania History of the Crusades* (1955) and, with Carlton J. H. Hayes and Charles Cole, in *A History of Western Civilization* (1949). In 1940-41 Baldwin served a term as president of the American Catholic Historical Association.

Although from infancy Marshall Baldwin's environment was that of college and university, his interests were in no sense narrow or confined to scholarship. In response to a questionnaire he listed his hobbies as bird watching and carpentry, but his major love was music. He played the piano, violin, viola, and French horn and sang in the faculty glee club, mastering each new instrument as its lack made itself felt in either the University Heights Symphony or, later on, in the

Bronx Symphony and Bronx Opera orchestras. In retirement, during the last spring season of his life, he performed with both the latter organizations, continued to enjoy a daily swim from his home on City Island, and just before entering Roosevelt Hospital sent off a final scholarly work to the Princeton University Press.

JOSEPH REITHIER
New York University

WILLIAM THOMAS LAPRADE died on May 14, 1975. He was born on December 27, 1883, in Franklin County in Virginia, where his father was a miller. Although he attended county schools, his early education was largely informal. He entered Washington Christian College in 1904 apparently with the intention of becoming a Campbellite clergyman, an intention that he carried out for a number of years. After receiving a bachelor's degree in 1906, he entered the Johns Hopkins University to study history. In 1909 he received the degree of doctor of philosophy with a dissertation entitled, "England and the French Revolution, 1789-1797."

In the same year, Laprade was appointed instructor at Trinity College in Durham, North Carolina. He was probably the first full-time teacher of European history in North Carolina and made a reputation with innumerable undergraduates as an inspiring lecturer. When Trinity College became Duke University in 1924, he introduced a graduate seminar that required a research paper more demanding, the student's thought, than a master's thesis. Although denying any philosophy of history, Laprade believed that history could bring perspective to contemporary problems. He thus wrote articles on topical subjects, served as a "four-minute man" in local theaters during the First World War, and taught a special course designed to give newly drafted doughboys some idea of what that war was all about.

At Trinity College and then at Duke University, Laprade served in several administrative capacities. He was active on committees and worked with

the departmental chairman of many years, William K. Boyd, and then as chairman himself from 1938 to 1953, to build up the history department from a faculty of two or three prior to 1920 to one of more than twenty in 1953. He was an editor of the Trinity College and Duke University Press from its beginning in 1922 to 1926 and then was acting director of the press from 1944 to 1956, while also serving as managing editor of the *South Atlantic Quarterly*.

Laprade's scholarly reputation rested upon studies of eighteenth-century England—articles in leading professional journals and four books: *England and the French Revolution* (1909); an edition of *The Parliamentary Papers of John Robinson, 1774–1784* (1922); *British History for American Students* (1926); and *Public Opinion and Politics in Eighteenth Century England to the Fall of Walpole* (1936). Laprade never thought that history was easy to understand, and both his literary style and his argument reflected the lack of pattern that he tried to explain to his students. Yet his conclusions helped reshape the interpretation of English history and, according to Herbert Butterfield, were of “strategic importance” before “the emergence of the Namier school.”

In the 1930s Laprade became active in the American Association of University Professors. He was chairman of Committee A on Academic Freedom and Tenure from 1937 to 1942 and was then elected president of the association. In 1948 he again became chairman of Committee A, a position he held through the McCarthy era until 1953. Over the years his annual reports reflected a faith in the principles that freedom in an academic institution was an “indispensable condition” for seeking truth and that freedom could not “be maintained without security of tenure.”

After “retiring” in 1953 Laprade continued to edit the *South Atlantic Quarterly* for four years and to read, speak, and write. He completed three book-length manuscripts with the intention, if they should not be published, of leaving them in his papers. His wife, Nancy Hamilton Caffee, whom he had married in 1913, died in 1968. On March 4, 1975, he was taken to Duke hospital with pneumonia. On April 12, during the celebration of its fiftieth anniversary, Duke University awarded him the honorary degree of doctor of literature. His only child, Nancy Laprade Hamilton, represented him on that occasion. Laprade interpreted this honor as a recognition of those members of the faculty of Trinity College who had helped transform that small college into an internationally respected university.

RICHARD L. WATSON
Duke University

GERHARD MASUR, professor of history emeritus at Sweet Briar College, died suddenly in Lynchburg, Virginia, on June 21, 1975, in his seventy-fifth year. Born in Berlin in 1901, Professor Masur studied with Friedrich Meinecke at the University of Berlin, where he received his Ph.D. *summa cum laude* in 1925. While teaching at that university in 1935 he left Germany for Switzerland. From there he went to Bogotá in 1936. While adviser to the commissioner of education in Colombia he produced his great biography of Simón Bolívar, which was published in English by the University of New Mexico Press in 1948; a German edition appeared in Constance, Switzerland, in 1949. Meanwhile Masur had come to the United States in 1946 and was appointed professor of history at Sweet Briar College in 1947, where he taught until 1966 when he resigned to accept an appointment at the University of California, Berkeley.

In addition to his definitive biography of Bolívar, Masur wrote several other notable books. Among them are his dissertation on Julius Stahl; *Prophets of Yesterday: Studies in European Culture, 1890–1914* (1961); and *Imperial Berlin* (1971), commissioned by the Berlin Historische Kommission. He is also well remembered for his many articles and his papers delivered at meetings of historians in both the United States and Germany. At the time of his death he was working enthusiastically on another volume in European culture to be the sequel to *Prophets of Yesterday*. A lively thinker and writer, a fascinating teacher, and a good and loyal colleague and friend, Gerhard Masur will be greatly missed by his many students, colleagues, and friends throughout the United States and Europe. He is survived by his wife, Helen Gaylord Masur.

LYSBETH W. MUNCY
Sweet Briar College

EARL SWISHER, professor emeritus of the University of Colorado, Boulder, who died on June 5, 1975, belonged to the pioneer generation of American academic specialists on China. He was a student at Lingnan University at Canton in 1924 in time to interview Dr. Sun Yat-sen as a young American journalist, and he also met Chiang Kai-shek and Mao Tse-tung. He spent several years studying in Peking as well as teaching at Lingnan and later at the Academia Sinica on Taiwan, where he represented the Asia Foundation for two years. During World War II he served with the marines in the Pacific. His Harvard Ph.D. dissertation of 1938, published as an AAS monograph, *China's Management of the American Barbarians*, made available a body of translated docu-

ments and is still a basic work. He also wrote a textbook, *Today's World in Focus—China*, for use in junior high schools.

In 1935 he was appointed to begin the Asian Studies program at the University of Colorado; since that time he has influenced several generations of Asia specialists and stimulated an inter-

est in China and Asia. Earl Swisher was a vigorous and public-spirited teacher who will be missed by students, friends, and colleagues.

JOHN K. FAIRBANK
Harvard University

JOYCE C. LEBRA
University of Colorado, Boulder

Other Books Received

Books listed were received by the *AHR* between July 15 and October 1, 1975. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

GENERAL

- ALLEN, GARLAND E. *Life Science in the Twentieth Century*. Wiley History of Science Ser. New York: John Wiley & Sons, 1975. Pp. xxv, 258. \$9.95.
- ATHUSSE, LOUIS. *Politics and History: Montesquieu, Rousseau, Hegel and Marx*. Tr. by BEN BREWSTER. Atlantic Highlands, N.J.: Humanities Press, 1975. Pp. 192. \$9.00.
- ASHIE, GEOFFREY, et al. (eds.). *The Quest for America*. New York: Praeger, 1971. Pp. 298. \$15.00.
- BENJAMIN, JULES R. *A Student's Guide to History*. New York: St. Martin's Press, 1975. Pp. x, 122. Cloth \$8.95, paper \$3.50.
- BLOCK, JACK. *Understanding Historical Research: A Search for Truth*. Glen Rock, N.J.: Research Publications, 1971. Pp. 146. Grades 8-12.
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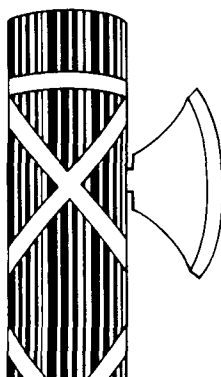
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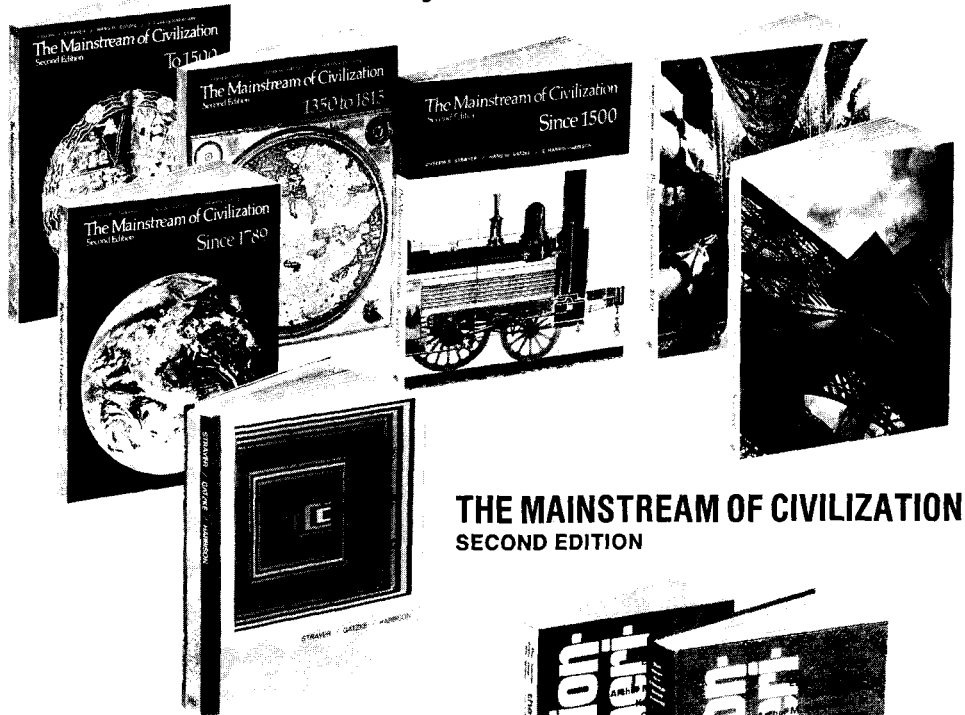
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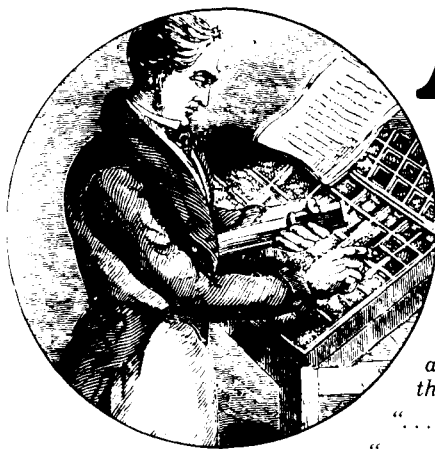
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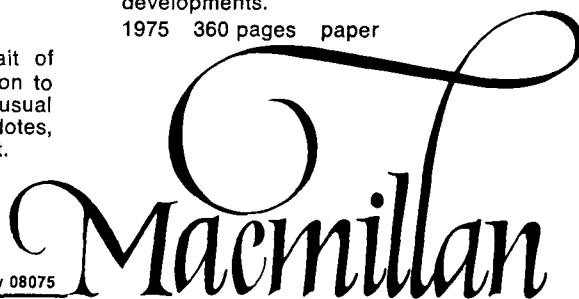
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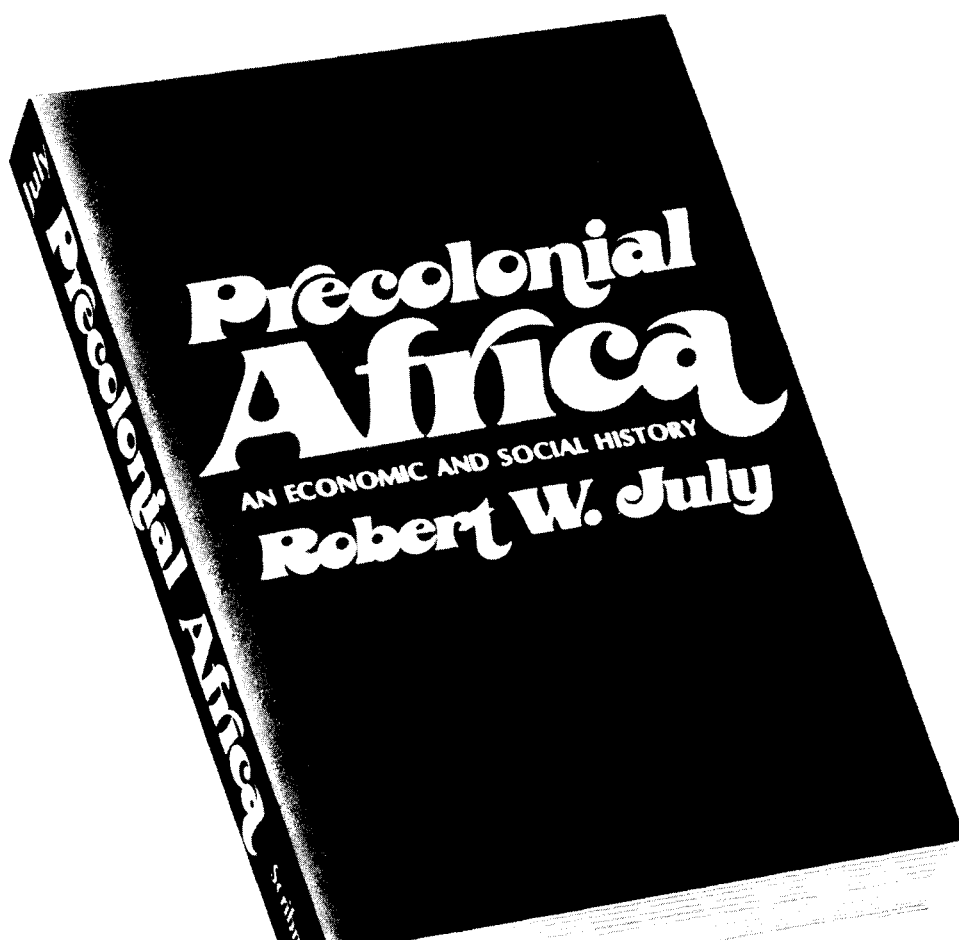
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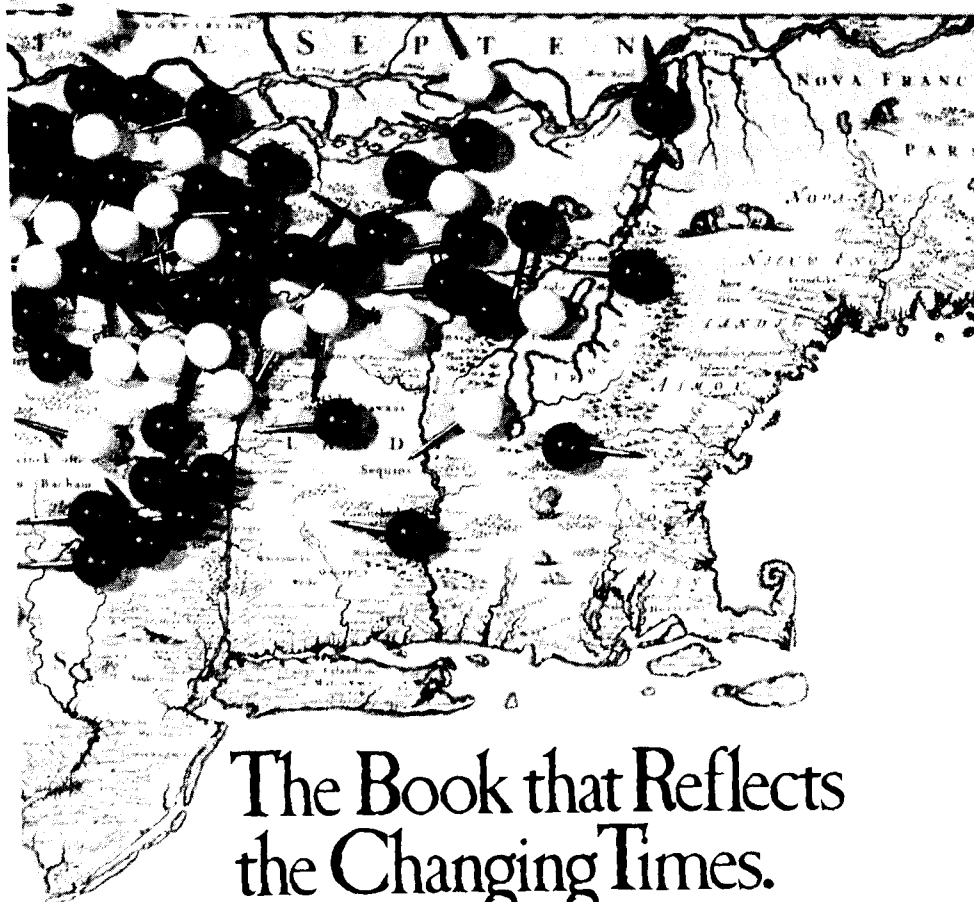
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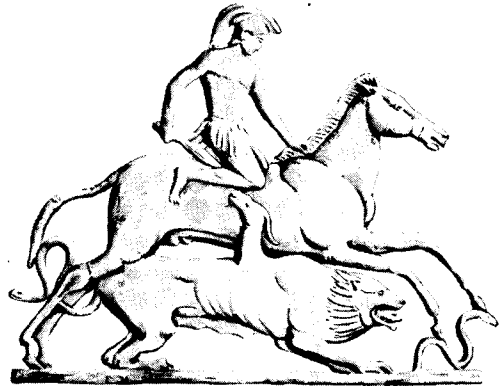
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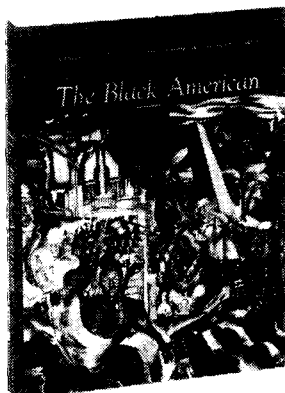
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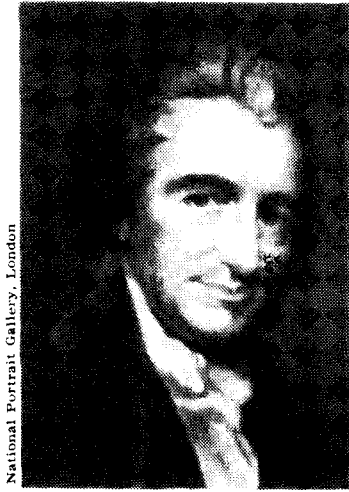
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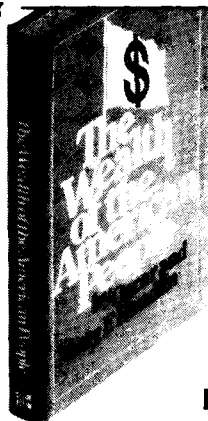
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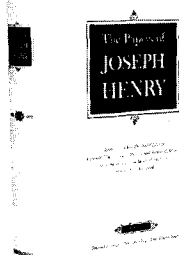
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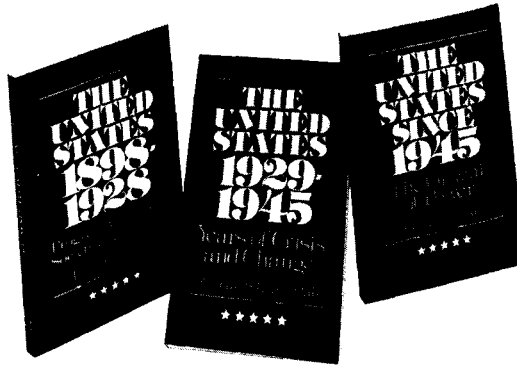
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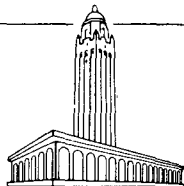
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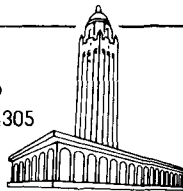
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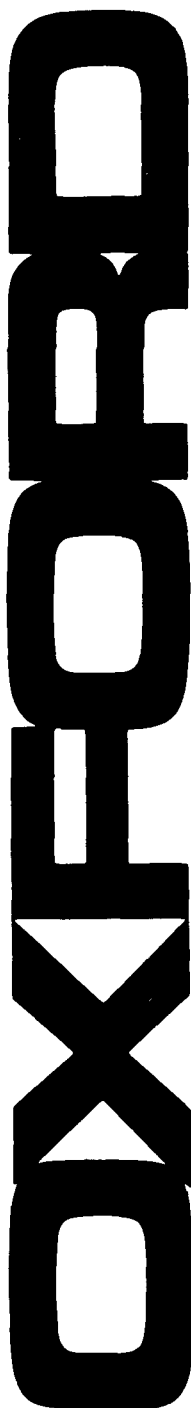
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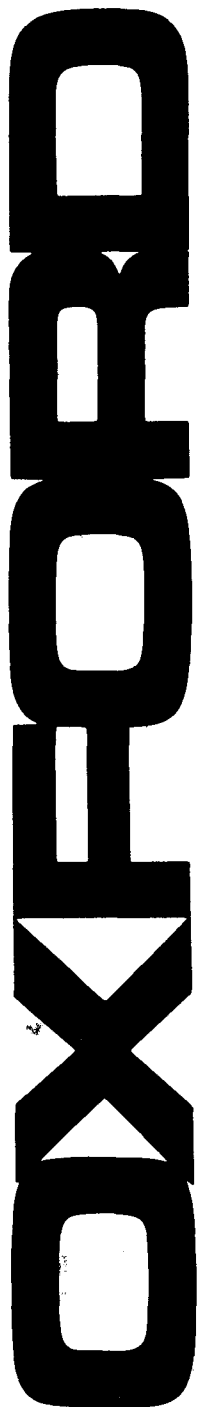
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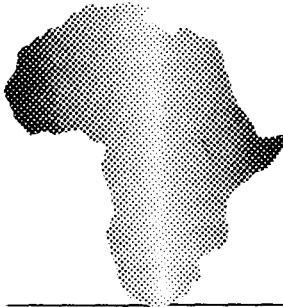
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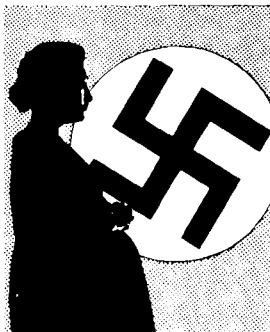


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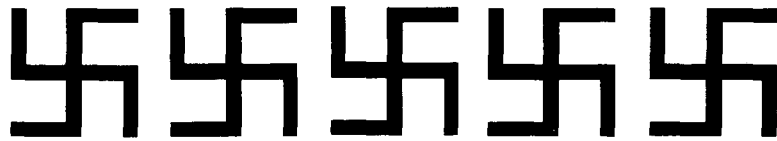
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